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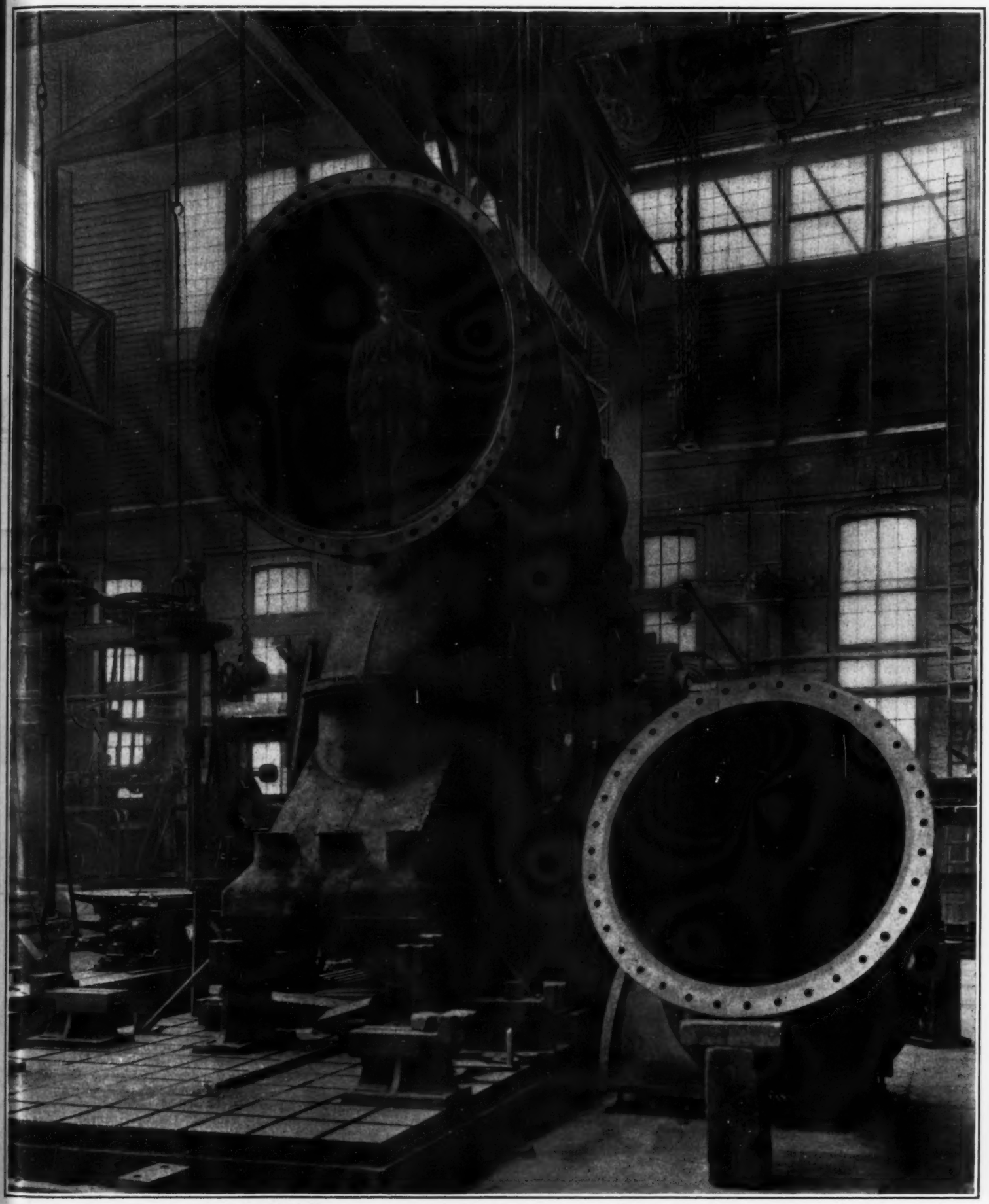


Used for drainage purposes at New Orleans, and can pump 168,000 gallons a minute.

THE LARGEST CENTRIFUGAL PUMP.—[See page 37.]

Ozone in Ventilation*

An Exposition of Faulty Methods of Investigation Heretofore Employed

By J. C. Olsen and Wm. H. Ulrich

In spite of the fact that a great many investigations have been carried out in recent years on the effect of ozonie on air bacteria and odors and also on the physiolog cal effects of ozone, the most diverse conclusions h ve been reached and opinions expressed with reference to the questions investigated. This confusion is due somewhat to faulty scientific technique and deductions from improperly chosen experiments as well as *ex parte* point of view. The most recent criticisms against ozone in ventilation are found in two articles which were published in the issue of September 27th, 1913 of the *Journal of the American Medical Association*, one by Jordan and Carlson and the other by Sawyer, Beckwith and Skolfield on the bactericidal, physiologic and deodorizing action of ozone. A number of errors in the methods used in the experiments given in these articles have been noted, and these seem so serious and the articles have been so widely quoted that it seems desirable to correct the misapprehensions which have been produced. Both of these articles refer to the fact that exaggerated claims were made by agents selling ozone machines.

In the article by Jordan and Carlson, it is stated, on page 16, that the concentration of ozone is determined by drawing the ozonized air through a solution of potassium iodide which has been acidified with sulfuric acid. The liberated iodine is then titrated with thiosulfate solution. It is well known among chemists that an acidified solution of potassium iodide is readily oxidized by the ordinary oxygen of the air, and, therefore, if an acidified solution of potassium iodide is used for the determination of ozone, the results will be high. The amount of the error will vary with the concentration of the ozone and may easily give results double the true concentration of ozone. This error can easily be demonstrated by drawing air free from ozone through such an acidified solution of potassium iodide. It is evident, therefore, that no reliance can be placed on the figures given for the concentrations of ozone which are reported in this article. In the article by Sawyer, Beckwith and Skolfield, the concentrations of ozone were not determined.

It is universally recognized by ventilating engineers who are familiar with the use of ozone that it is of the greatest importance to regulate the concentration of the ozone and that ozone is useful only when employed in the proper concentration. This well-known principle seems to have been so little understood by these investigators that they failed to make careful and accurate determinations of the concentrations of ozone used, and therefore many of the conclusions which they reached are entirely vitiated.

Another very serious error in experimental procedure is found in the tests which were made on the effect of ozone on odorous substances. A considerable number of such substances were experimented with, and the conclusion was reached that the ozone *masks* these odors but does not destroy them, and that, therefore, ozone is not useful in the removal of such odors.

The method of procedure consisted in exposing the substance giving off the odor until a marked odor was noticed in the small closed room which was used for the experiments. The ozone machine was then operated until a strong odor of ozone was produced. Observations were made from time to time of the odor in the room and it was observed in a good many cases that the ozone odor gradually disappeared and the odor of the substance experimented upon returned. In some cases ozone was again generated until its odor was pronounced, and observations again made with reference to the disappearance of the ozone odor and the reappearance of the odor of the substance experimented upon. The conclusion was drawn that the ozone did not destroy the substance giving the odor but masked it; this conclusion was based upon the disappearance of the ozone odor and the return of the other odor. No other evidence, whatever, on this point is presented.

In these experiments no attempt seems to have been made to determine the amounts of the odorous substances which were present in the air except by the odor. The experimenters apparently did not consider the fact that the odors of substances differ a great deal in intensity, and that the quantities of substances which would be present, even though the intensity of the odor was the same, would differ very much. These authors also failed to keep in mind that the destruction of odors by ozone is an oxidizing process, and that this, as well as all chemical reactions, is quantitative in the sense that a definite amount of oxygen is required to oxidize a definite amount of an oxidizable substance.

The following reaction takes place when ozone oxidizes hydrogen sulfide:

$$H_2S + O_3 = H_2O + S + O_2$$

That is, 34 parts of hydrogen sulfide would require 48 parts of ozone for their oxidation. When the hydrogen sulfide is dissolved in water the sulfur liberated is still further oxidized by the ozone to sulfuric acid, which would require a still larger quantity of ozone, but according to the reaction given a somewhat larger amount of ozone than hydrogen sulfide would be necessary for the destruction of this substance. Now, if the intensity of the ozone odor is much greater than that of the odor of hydrogen sulfide, the hydrogen sulfide would be oxidized by the ozone in the experiments reported by Jordan and Carlson, and some other authors quoted, and the hydrogen sulfide odor would then return as reported by these investigators.

In order to verify these conclusions experiments were carried out to ascertain the amount of hydrogen sulfide which will give a distinct odor. A large balloon flask of 30 liters capacity was used. The hydrogen sulfide was produced by treating known weights of carefully analyzed iron sulfide with dilute sulfuric acid. The reacting substances were placed on a watch crystal suspended in the center of the balloon flask. In addition to the odor, tests were made with lead acetate paper.

INTENSITY OF ODOR OF HYDROGEN SULFIDE.

Mg. H_2S per Cubic Meter	Test with Lead Acetate Paper	Odor
977	Very black	Very strong
244	Very black	Strong
61	Very black	Distinct
30	Turned black slowly	Fairly distinct
15	Brown on edges	Faint
7.6	Turned brown very slowly	No odor

While to obtain a distinct odor of hydrogen sulfide, 61 parts are required, the odor of ozone is very marked when present to the extent of one part per million, the limit being one-tenth part per million. In the experiments of Jordan and Carlson, the concentration of the hydrogen sulfide must have been from 30 to 60 milligramme. One part of ozone would have given a strong odor which could mask the odor of the hydrogen sulfide until, by the oxidation of the latter, the ozone was decomposed. Less than one part per million of the hydrogen sulfide would be destroyed by this oxidation, leaving a sufficient amount of hydrogen sulfide to give a very distinct odor. On again generating ozone until a strong ozone odor was obtained, the hydrogen sulfide would be again "masked," and when the ozone odor had disappeared the hydrogen sulfide odor would reappear. This could be done repeatedly, as reported by Jordan and Carlson. The conclusion which they drew, however, is entirely unjustified, namely, that their experiments showed that the hydrogen sulfide odor was merely masked and hydrogen sulfide not oxidized or destroyed by the ozone.

In order to verify this conclusion, the following experiment was carried out; A concentration of 25 milligrammes of hydrogen sulfide was treated with ozone of a concentration of 35.6 milligrammes per liter. In these concentrations there would be just enough ozone to oxidize the hydrogen sulfide. In this experiment the ozone odor was at first very pronounced but after this odor had disappeared there was no hydrogen sulfide odor. A slight acidity was indicated by the reddening of blue litmus paper. The ozone used had been very carefully tested for nitrous oxides, but none was found.

Another experiment was carried out in which the ozone concentration was 7.6 parts per million, while the hydrogen sulfide concentration was 60.7 milligrammes per cubic meter, so that only a small part of the hydrogen sulfide could be oxidized by the ozone present. At first only the odor of the ozone could be detected. The hydrogen sulfide odor gradually returned so that within one hour a faint, and after two hours a distinct, hydrogen sulfide odor was detected, while the ozone odor had entirely disappeared.

This experiment could be repeated three or four times, as reported by Jordan and Carlson on page 33 of their article. They further state: "The mechanism of this masking action of ozone does not concern us here." If the authors had considered the "mechanism" of this action they might have reached entirely different conclusions and would have seen that their experiments were in exact accordance with the theory that ozone oxidizes hydrogen sulfide and other substances. They still further refer to the fatigue of the olfactory endorgans by the ozone. They say "Strong concentrations of ozone rapidly fatigue or anesthetize the olfactory epithelium." One wonders why the authors did not make this statement general, and state what every chemist has frequently observed, that *hydrogen sulfide* and the numerous other odors which are present in chemical laboratories produce the same effect on olfactory epithelium, so that these odors are not noticed by workers in the laboratory.

Hydrogen sulfide is also oxidized by the air, as is shown by the fact that the odor slowly disappeared in a duplicate experiment in the absence of ozone.

In some cases ozone acts as a catalytic agent. This was shown by the action of ozone on linseed oil. Weighed quantities of linseed oil were exposed to air and ozone. The oil exposed to air gained 17 milligrammes, while an equal quantity exposed to the action of ozone gained 110 milligrammes during the same time. The amount of ozone generated was 11.4 milligrammes. The ozone, therefore, acted as a catalytic agent, causing the absorption of 82 milligrammes of oxygen, which is nearly five times as much oxygen as was absorbed by the oil exposed to air alone. It is reasonable to suppose that ozone would act as a catalytic agent and cause the oxidation of other oils and organic substances similar to linseed oil.

In the case of ammonia the same considerations apply. Ammonia is oxidized by ozone in accordance with the following equation:

$$2NH_3 + 3O_3 = N_2 + 3H_2O + 3O_2$$

In this case, one part of ammonia is oxidized by about four parts of ozone. A study of the intensity of the odor of ammonia gave the following results:

INTENSITY OF THE ODOR OF AMMONIA.

Mg. NH_3 per Cubic Meter	Test with Litmus Paper	Odor
1,000	Turns blue readily	Strong
659	Turns blue slowly	Fairly strong
329	Turns blue slowly	Fairly strong
165	Turns blue very slowly	Fairly strong
82	Turns blue very slowly	Quite distinct
41	Turns blue on edges very slowly	Faint
21	Turns partially blue on edges very slowly	Very faint
10	No action	No odor

Experiments were carried out in which known amounts of ammonia were treated with definite amounts of ozone, and the ammonia remaining was determined by absorption with sulfuric acid and nesslerizing. Ammonia is acted upon very slowly so that 24 hours were allowed for the reaction. The concentration of the ozone was 35.6 milligrammes, and of the ammonia 125 milligrammes per cubic meter. After 24 hours, 78 milligrammes of ammonia remained in the flask containing air and 75 milligrammes in the flask containing ozone. The quantity of ozone present was sufficient to oxidize 8½ milligrammes of ammonia per cubic meter. The experiment indicates some oxidation of ammonia by ozone. Erlandsen and Schwartz[1] state that their results showed no action of ozone on ammonia.

Experiments were also carried out to ascertain the intensity of the odor of oil of cloves. It was found that 66 milligrammes per cubic meter would give a strong odor. The amount of ozone necessary to oxidize oil of cloves cannot be calculated exactly, but it would probably require several times more than an equal weight. On subjecting the oil of cloves vapor in a concentration of 36.6 milligrammes per cubic meter to the action of ozone of a concentration of 33 milligrammes per cubic meter, it was found that at first a distinct odor of ozone could be detected, which gradually disappeared and was replaced by a sweet odor which had no resemblance to the strong odor of the oil of cloves. Very evidently the oil of cloves, or one of its strong smelling constitutents, is oxidized, at least partially, so as to leave an organic compound having an entirely different odor. The flask was allowed to stand a total of 21 hours, although the reaction was practically complete within 2 to 3 hours. The reaction did not seem to be entirely regular. The experiment was repeated several times and the formation of the sweet aromatic substance repeatedly observed, but at times the odor of cloves persisted. Any excess of ozone was removed by shaking with 10 per cent ferrous sulfate solution. Control experiments were also made with a balloon flask containing oil of cloves and air only. Erlandsen and Schwartz have made a similar observation with respect to skatol and indol. They state that these substances are completely decomposed by ozone with the formation of pleasant smelling substances similar to coumarin. They also state that mercaptan is rapidly decomposed by a large excess of ozone.

*Read at the 6th Semi-annual Meeting of the American Institute of Chemical Engineers, Troy, New York.

[1] *Zeit. of. Hyg.*, 1913, pp. 81-100.

These authors also found that hydrogen sulfide is oxidized by ozone. Jordan and Carlson also mention the work of Erlandsen and Schwartz, but state that the results of the latter agree with those of Jordan and Carlson in showing that ozone *masks* odors and does not oxidize the substances discussed. Jordan and Carlson even state that "The inability of ozone to oxidize (to any appreciable extent) ammonia vapor and oil of cloves is very striking." Franklin gives the results of experiments showing the oxidation of a great many organic substances, and demonstrated that carbon monoxide is oxidized to carbon dioxide.[2]

Undoubtedly many of the conflicting conclusions which have been reached are due to the failure of experimenters to take into account the quantitative relations between ozone and the substances to be oxidized, and have generally failed to realize that on account of the much greater intensity of the odor of ozone than that of other substances producing odor far too little ozone has been employed in the experiments. The odor of ozone is at least 100 times as intense as that of other substances having a pronounced odor.

The common method of using ozone to destroy odors seems to be justified by these considerations. The ozone machines in good practice are operated so that a very small concentration of ozone in the air is produced. The continued renewal of this small amount of ozone oxidizes the odorous substances and gives the total quantity of ozone required by chemical theory.

Jordan and Carlson undertook to prove that ozone does not destroy smoke by coating a piece of glazed paper uniformly with a thin film of lampblack and subjecting the carbon to the action of ozone for 10 hours, and state that there was no effect on the lampblack. They regard this as evidence that ozone does not destroy smoke although they state that smoke also contains carbon monoxide, sulfurous acid, etc. They seem to have made no attempt to study the effect of ozone on carbon monoxide and sulfurous acid, but state that the assertion that "ozone destroys smoke" is equivalent to a deliberate deception because ozone does not oxidize carbon particles suspended in the air. Not being chemists, Jordan and Carlson might not have known that carbon is extremely difficult to oxidize, but they should have known that carbon monoxide and sulfurous acid, as well as creosote and other pungent and aromatic organic substances present in smoke, produce disagreeable and toxic physiological effects. It is just these constitutents of smoke which are easily oxidized by ozone. The carbon which is not acted upon is totally inert and harmless.

Jordan and Carlson also carried out investigations on the action of ozone on air bacteria. As has already been stated, their determination of the concentration of ozone cannot be relied upon because acidified solutions of potassium iodide were used which give high results. The authors admit that the plate method which they used was not an exact one, and that therefore the results were irregular. They obtained a reduction of bacteria from 64 to 38, 49 to 18, 61 to 67 (increase), 78 to 34; that is, the bacteria were reduced to 40.6 per cent, 63.3 per cent and 56.4 per cent in three tests, while in the fourth there was an increase to 109.8 per cent. Most authors would discard the fourth test and consider the reduction to be about 55 per cent. Jordan and Carlson then tried Winslow's more exact method, but did not use a large enough sample for the test (4.5 liters), so that the highest number of bacteria counted was 7. It is not good practice in bacteriological work to rely on counts of so small a number. Jordan and Carlson draw the following strange conclusion from their results:

> The alleged effect of ozone on the ordinary air bacteria, if it occurs at all, is slight and irregular even when amounts of ozone far beyond the limit of human physiological tolerance are employed."

There is no reason to suppose that the ozone used in these experiments was "Far beyond the limit of human physiological tolerance." The reduction in the number of air bacteria is not "slight and irregular." The experimental results of Jordan and Carlson agree with the results obtained by one of us[3] in New York school rooms showing a reduction in air bacteria and moulds of 75 per cent, 91 per cent and 91 per cent, the greater reduction resulting from longer exposure to ozone. No ill effects were observed on the children and adults present during these tests.

Jordan and Carlson (p. 34) state: "Some bacteria are undoubtedly killed by ozone, especially if they are in a moist condition." This statement is correct as it is generally recognized that ozone destroys moist bacteria very rapidly. The conclusion drawn by Jordan and Carlson is very far from being correct. They say: "In practice, however, the fact is of very slight importance." The works of Chapin, Doty and of Winslow and Robinson have fairly disproven the belief held so long that bacteria existed as a menace in rebreathed air. It has been shown that infection occurs but rarely from air-borne bacteria, and then only when the *bacteria are in the moist condition*. It is just these virulent bacteria which are quickly destroyed by ozone even in low concentrations. The bacteria which resist the ozone are powerless to transmit disease.

It is by no means necessary to show that ozone is capable of sterilizing the air in order to show that it is useful in ventilation. There is in fact no other disinfectant which can be used even in low concentrations in living rooms. All other known disinfectants are highly dangerous in concentrations high enough to be at all effective. Ozone in low concentrations will both remove odors and will materially reduce the bacteria content of the air.

Jordan and Carlson attempted to show that ozone is dangerous by subjecting guinea pigs to high concentrations until the animals died. They also forced strong ozone (10 parts per million) directly into the lungs of dogs and rabbits after performing tracheotomy under ether and inserting a tube well below the larynx and treating the wound with cocaine. They say they did this because at least three-fourths of the ozone is decomposed by the mucous membrane of the respiratory passages. It is difficult to see what bearing this experiment can have on the use of ozone in ventilation. The ozone which was forced into the lungs must have been several hundred times as concentrated as it is ever used in ventilation. It would have been quite as logical to place animals in pure oxygen, or even an atmosphere of 50 per cent oxygen and also force these gases directly into the lungs. When the animals died and the lungs were found to be inflamed the conclusion might be drawn by highly academic experimenters that it is dangerous to breathe air containing 20 per cent oxygen.

Jordan and Carlson carried out experiments with what might be called low concentrations of ozone. They subjected 4 cats, 4 rabbits, 6 guinea pigs and 12 rats to ozone of 1 part per million nine hours daily during two weeks so that irritation of the eyes and nose was produced. Body weight, appetite and general condition were noted. No ill effects on appetite and body weight or general condition could be observed. The conclusion could, therefore, be drawn that ozone in moderate concentrations is harmless, but Jordan and Carlson warn us that this conclusion is not justified.

They say: "We desire to state, however, that this test does not warrant the conclusion that the ozone in concentrations that may be used in practical ventilation is harmless to man. Two weeks is a short time in the life of a man. If ozone in ventilation should come into general use, it would mean in the case of office and shop workers exposure to ozone from 6 to 10 hours a day, 6 days of the week, from 9 to 12 months of the year for from 20 to 50 years. And even if this prolonged exposure to ozone should prove harmless to the robust person, what about the unfortunate person whose lungs have only slight power of resistance?"

Jordan and Carlson seem to have fallen into the error of assuming that because they have not tried to, and therefore have not demonstrated actually, benefit from the use of ozone, this is equivalent to having demonstrated the reverse, i. e., the harmfulness of ozone.

If the facts presented in this paper are properly interpreted they will be found to be in accordance with the view that ozone is a powerful disinfectant and deodorizing substance, which, in suitable concentration, is without any injurious effects whatever. The elimination of odor is by no means the least important function of ozone and there is no other agency available except dilution with fresh air. In many cases it is impossible to introduce enough air for this purpose without producing annoyance and dangerous drafts of air, not to mention expense of blower installation and operation as well as heating the air. As a matter of fact, before ozone was available, disagreeable odors have often been considered unavoidable nuisances which could not be eliminated or overcome.

With reference to the alleged harmful effects of ozone, no single instance of harm to a person from the proper use of ozone in ventilation has been published, but all adverse opinions have been deduced, by inference, as in the paper by Jordan and Carlson, from experiments performed with very high concentrations, while all efforts to produce harm experimentally with weak ozone have failed.

Jordan and Carlson report that 26 animals, exposed for 14 days, during 9 hours each day, to concentrations high enough to cause irritation of the eyes and nose, suffered no ill effect whatever. Hill cites the cases of the numerous workers in the London underground tubes who have shown no ill effect in three years. Gminder cites the unharmed workers in the spinning mills at Reutlingen, and numerous similar instances of prolonged proper use of ozone without a single complaint are today in existence. The Jordan and Carlson report is the most elaborate and convincing laboratory test that has been published.

[2] *Heating and Ventilating Magazine*, 10, No. 11.

[3] "Purification of Air and Water by Means of Ozone," Olsen, Fourth International Congress of School Hygiene.

Report of the Bureau of Mines

THE annual report of the United States Bureau of Mines, issued by the director, Joseph A. Holmes, discloses a surprising amount of useful work accomplished, and clearly indicates the inestimable value this bureau could be to the country if sufficient funds were available to enable it to carry out the work that is clearly necessary.

Broadly the lines of work of the greatest national concern devolving on this bureau are the safeguarding of the lives of miners and of the employees in the metallurgical and mineral industries, and the development of a more efficient and less wasteful preparation and use of our mineral resources, and while the latter of these is of vast monetary concern it is the former that will appeal most strongly to popular interest. One of the first directions in which this work was prosecuted was in relation to safety in coal mines, and a large number of details have been investigated with good results. Among these are the studies that have been made as to the causes of explosions, and their prevention; the use of explosives and electricity in mines, ventilation and rescue work, as well as many other points. In the mine rescue work six mine-rescue stations have been established, as well as eight rescue cars and one motor truck, all of which have rendered most valuable aid in cases of accidents; and the bureau itself has been directly responsible for the saving of a number of lives. One of the most valuable and permanently useful branches of this work has been the instruction given to miners, and during the year 2,826 miners were trained in rescue work and 5,780 were trained in first aid methods. At the end of the year 24,975 men had been instructed altogether, and through their means a knowledge of rescue work is spreading rapidly. Other investigations looking to making mines safer are under way, but much more could be done if greater resources were available.

The importance of the work that can be done in the prevention of waste may be understood from the statement that the total wastes or losses in mining and utilizing our mineral resources now amount to more than $1,000,000 a day, and when it is realized that these losses are irreplaceable the desirability of prompt action is apparent. That the bureau is doing splendid work in this direction is shown in a single item, for by the expenditure of less than $15,000 a saving of natural gas has been made of not less than $15,000,000, or more than the bureau has cost since it was organized.

A careful estimate indicates that in the mining of 600,000,000 tons of coal during the last calendar year there was wasted or was left underground in unminable condition 300,000,000 tons of coal, and it is believed that more than half of this loss is preventable. Of petroleum not less than $50,000,000 in value is annually wasted by present methods. In the ordinary process of coking coal $75,000,000 is lost annually. These are but indications of the work that should be done, and the directions in which the bureau is employing its activity, and there are multitudes of others requiring attention. One interesting study undertaken by the bureau was in regard to radium, uranium, and vanadium with a view to developing methods for the cheaper extracting of these valuable metals from their ores, both with a view to reducing the price and to develop the radium industry in this country and stop the exportation of our ores to foreign countries, where they are now treated. As a result of this study, which was undertaken in connection with the National Radium Institute, methods for the extraction and purification of radium, vanadium, and uranium from carnotite ore have been devised. The methods have given satisfactory results and indicate radium can be produced at a much lower price than that at which it is sold at present.

While in some quarters the narrow view is taken that anything that assists mining or metallurgy in this country will be only for the advantage of some corporations, the reading of this report is convincing that the field covered by the Bureau of Mines is to the direct advantage of every citizen of this country, no matter who may benefit incidentally, for it is the public that pays eventually for every waste, and the cheaper and more economical production can be made in any direction the lower will be the price at which the goods can, and must be sold; and it is becoming more evident every day that no time should be lost by the Government in extending to our mineral industries and manufactures the aid that is so liberally given to many other departments that are conducting valuable investigations of our resources. Incidentally it may be remarked that the policy of withholding aid from the mining interests for fear it may assist some corporation is likely to be the surest way to create monopolies, for the small mine owner cannot afford to develop economical processes that will enable him to compete with the big concerns, and if the Government does not help him in this way his only resource is to transfer his property to the large corporations.

The Diary of Kilauea*

Volcanic Action in Hawaii Being Observed and Recorded

An important contribution to scientific knowledge is that just published by the Society of Arts. The institute has recognized the need of systematic observation of volcanoes, and for the purpose has established in the Hawaiian Islands an observatory, where, with most recent methods and equipment, the facts that the crater has to offer are to be collected. The work owes its initiative to the interest and activity of Prof. T. A. Jaggar, Jr., of the Geological Department, who is the director of the Hawaiian Volcano Observatory.

The report of the Hawaiian Volcano Observatory is a neat quarto of seventy-five pages, well illustrated, which gives the history of the institution and its work up to and including 1912.

Copyright Chock Chong, Hilo

Crater of Halemaumau, about January 4th, 1912.

In 1912 the observatory was put on a five-year foundation, and early in the year the present building was constructed. Dr. Jaggar was named head of the observatory and relieved of his duties in Boston for the purpose of making investigations at the volcano of Kilauea, where the station is located.

The institute has the lease of a tract of three acres on the brink of the crater, with the option of renewal, and its station includes living rooms, administration offices and work rooms, while the Whitney Laboratory of Siesmology is a basement room of concrete, floored on the solid ledge of basalt. The place of the instrument house is most striking, being on the very edge of the rim, where at times the clouds from the crater encompass it. During some of the experiments it has been necessary to establish a line of assistants who by calls from one to another directed the manipulation of the instruments.

The story given in the report is largely a day by day account, and valuable for scientific purposes. The conduct of the fiery lakes in the bottom of the crater is chronicled, the oscillations of the lakes within their basins, the different kinds of action, the fountains, one of which, "old faithful" was playing at intervals of thirty seconds, sending fiery spray to one hundred feet in height, while the earthquake shocks of every little while are noted.

Experiments were made in gas-composition of the vapor clouds above the lakes, the flows of molten lava into fiery pools are described and the floating island. New cones on the floor of the great crater are a phenomenon of interest, the fall of the crater walls, the range of the fire with reference to surface, are features in an activity that knows no cessation.

Part of the work was that of Perret and Shepard, the former the well-known vulcanologist and the latter detailed for the work by the Carnegie Institution. In this series of observations, a cable was stretched across the lake, and from it the thermometers were lowered into the lava to ascertain its temperature. It was a very difficult performance, and one after another of the instruments were lost, on account of the heat and acid condition of the vapors, which melted or corroded the wire ropes. One record was obtained, however, at 1,800 deg. Fahr., and a moment after the wire ropes were melted and the instrument lost. It was the third pyrometer thus to be destroyed, but the observation is considered to be a good one of the temperature of Kilauea lava.

One of the striking matters presented by this volume is a bit of prophecy. Dr. Jaggar thinks that there is a rhythmic escape of lava, which has been fairly well verified by the records of past eruptions. Mauna Loa, which is the subject of this prediction, seems to have decreased the duration of its eruptive periods, which previous to 1868 were eleven and one half years long, and since that date have been five years long. The time between these periods when the volcano has remained quiet has decreased from five and one half years to four and three quarters. Applying these figures to the last eruption, Dr. Jaggar is looking for renewed activity in this volcano in February, 1915. There is really no satisfactory information on which to predict the month, but from the usual conduct of the volcano, February seems the most probable. It will be of great interest to know whether this prediction, made in September, 1912, is fully realized.

* Courtesy of *Science Conspectus*.

Food for Polar Explorers

In a recent article in the *Daily Telegraph*, London, Sir Ernest Shackleton discusses the important question of food supplies and the proper diet for explorers in polar regions, and as these are questions of vital importance in regions where temperatures not far short of 80 degrees below zero are common, as the success of such expeditions depends primarily on the health of the explorers, this article is quoted in full and will be found of general interest, particularly the tables giving the scale of food rations that has been prepared by Colonel Beveridge, of the Royal Army Medical College, particularly for this expedition:

"To provide the best kind of nourishment under the long and, even at their best, somewhat trying conditions of the Polar regions is a matter of considerable thought and anxiety in the organization of such an expedition as ours. Captain Scott, whose great achievement and tragic death are ever fresh in mind, gave the matter his most careful consideration. But I believe this is the first occasion when Polar explorers will have the benefit not only of practical experience, but of scientific experiments and tests which should prove of very great advantage in the coming effort.

"The sledging distance to be covered will be roughly 1,800 miles, and the first half of this, from the Weddell Sea to the Pole, will be over unknown ground. Every step will be an advance in geographical science. It will be learned whether the great Victoria chain of mountains, which has been traced from Ross Sea to the Pole, extends across the continent, and thus links up (except for the ocean break) with the Andes of South America; and whether the great plateau around the Pole dips gradually to the Weddell Sea. Continuous magnetic observations will be taken on the journey. The route will lead to the Magnetic Pole, and the determination of the dip of the magnetic needle will be of importance in practical magnetism. The meteorological conditions will be carefully noted, and this should help to solve many of our weather problems. The glaciologist and geologist will study ice formations and the nature of the mountains, and his report should prove of great scientific interest. This apart from the scientific work of the base parties, one of whom I shall leave on the Weddell Sea and the other of whom I hope to meet on the Ross Sea at the conclusion of my long march.

"On this march, which is roughly 1,800 miles, I shall have the company of Mr. Frank Wild, my second in command, who served with distinction during an extended sledge journey during the National Antarctic Expedition, 1901-4, and who was one of the southern party of my last expedition (1907-9), together with four picked men. Given favorable conditions we hope to do the journey in 90 days; but we shall take enough food with us to last 120 days, in case we meet with blizzards which confine us to our tent. The average duration of a blizzard is two or three days, but a particularly bad storm has been known to last as long as 17 days. Even, therefore, if we meet with the worst weather conditions, we shall have plenty of supplies to the good. In the matter of temperatures the mean Antarctic winter temperature is always below zero. In summer it is about —28. On the plateau it is 40 degrees below zero, but in the spring months the temperature falls as low as 75 below zero.

"Now several important considerations have to be borne in mind in selecting food supplies for conditions such as these. The food must be wholesome—it must be uncontaminated—and nourishing in the highest degree. Only by the most nutritious and (as far as possible) most varied food can one hope to hold the dread disease of scurvy at bay. There was a time when scurvy, which is often produced by eating preserved food in an unwholesome condition, used to be regarded as the inevitable corollary of a prolonged stay in Polar regions; it has been the *bete blanche* of more than one heroic effort. On the 1907 expedition we had not a single case of scurvy attributable to the food we had brought with us.

"Again, food taken with us on our sledging expedition must be as light as possible in weight, and yet, contradictory as this may seem on the face of it, it must be

Photo by E. Moses.

Hawaiian volcano observatory. April, 1912.

Photo by E. Moses

Crater of Halemaumau, September 19th, 1909, looking southwest, showing traveling fountains, spatter rampart and flows.

substantial. Excessive concentration not only diminishes its nutritive value almost to nil, but renders it less easy of assimilation. Bulk is as essential as nutritive value. In very low temperatures the heat of the body, which is the life of the body, can be maintained only by use of fatty and farinaceous foods in such liberal quantities as circumstances allow. Then, again, sledging food supplies must not require much cooking. Something that can be heated easily and quickly is the desideratum, for the amount of fuel that can be carried is strictly limited, and if it runs out there are no means of replenishing it. Failing the means of heating the food it must be of a character that can be eaten without cooking at all. In winter quarters—at the base camps—a great variety of food is possible.

"On our sledging journey, to which I have already referred, our rations will be 35 ounces per man per day, containing 5,512 calories. In normal life an ordinary man eats about 3 pounds of food a day, containing some 2,500 calories, so that we shall have the benefit under Polar conditions of some 3,000 calories per man.

Here are the sledging rations, in total, we are taking with us, calculated to last for 20 men, for 100 days per man:

	Lbs.
Unhulled Scotch oatmeal, fine ground	500
Lard (pure)	1,000
Sugar (ground centrifugal)	150
Sugar (loaf)	300
Beef powder	375
Glidine	125
Biscuits (Antarctic, H and P.)	875
Trumilk	300
Nut food (6 ounce packets)	750
Lime juice (concentrated, made into lozenges or capsules)	65
Cerebos salt (in packets containing 0.75 ounce)	16
Meat extract (in 5 ounce cubes)	65
Tea	32
Total	4,553

"The whole of it is being packed in oblong boxes of 60 pounds each (oblong because it is easier to handle and balance this shape). The boxes are made of Venesta wood, which is both light and durable. Everything in the nature of meat is in skins, which come in useful for feeding the dogs, although, if necessary, we shall eat the skins ourselves. I hope it won't be! These rations are going out in a refrigerator, and will remain in refrigeration until they arrive at their (first) destination in the land of the Southern Cross. Among the articles mentioned in the tables given herewith, the lime juice has been concentrated down at a temperature of not more than 95 deg. Fahr., so that the anti-scorbutic properties are thereby preserved. Glidine is a vegetable proteid, while Trumilk has this advantage over ordinary dried milk, that it has not been subjected to a heat so great that the vitamines are thereby destroyed.

"It will be seen that we are to have a total of 11½ ounces for breakfast. While in Norway recently we found that we could not eat two-thirds of that for our matutinal meal, and, I fancy, there are few men in England, even among the laboring classes, who are subjected to strenuous work, who could manage as much for the morning meal.

"Our only stimulant will be tea. We shall have a small quantity of brandy with us, but only for medicinal use in emergency. It often happens that in up-lifting the arms to fix the tent for sleep the blood rushes from the hands, and the fingers become instantaneously frost-bitten. A man is then quite *hors de combat*, but a drop of brandy will help to give a fillip to the blood in the affected parts.

"In the ordinary way breakfast is taken at 6 A. M., luncheon at noon, and supper at 6 P. M. But to achieve the object of our sledging journey we—I mean myself Wild, and our four comrades—propose to split up the 24 hours into a 19 hours day. That will give us 8 hours' marching, 8 hours' sleep, and 3 hours for the preparation and taking of meals. As far as possible our 'days' and 'nights' will be arbitrarily fixed in this fashion, but our speed necessarily depends upon a variety of circumstances.

"Our last supply of fresh meat and vegetables will be taken on board at the Falklands. It may be possible to carry enough to last us a fortnight. After that we shall take to our special supplies.

"We are also taking some soil and seeds, and hope to grow just a small quantity of oats, barley, rape, mustard and cress, and peas. As the green shoots come up they will be cut and made into sandwiches. We may be able to take a few of these sandwiches with us for a very special treat on our 1,800-mile tramp."

The valuable tables of the sledging rations worked out by Colonel Beveridge are as follows:

SCALE OF RATIONS.

Article and Amount.	Moisture.	Mineral matter	Protein.	Fat.	Carbohydrates.	Calories.
BREAKFAST.						
Oatmeal, 2 oz.	4.03	0.88	12.94	5.34	33.02	236
Lard, 3½ oz.	.....	.....	.....	99.23	.....	923
Sugar, ½ oz.	.....	.....	.....	.....	14.17	58
Beef powder, 1½ oz.	0.99	3.85	35.29	2.37	.....	166
Glidine, ½ oz.	1.28	0.08	11.78	0.14	0.82	53
Biscuits, 1 oz.	3.52	0.30	9.21	0.85	14.45	105
Trumilk, 1 oz.	0.13	1.66	6.70	8.00	11.58	150
Sugar (additional) lump, 1½ oz.	.....	.....	.....	.....	42.51	174
Total 11½ oz.	9.95	6.77	75.92	115.93	126.55	1,865
LUNCHEON.						
Biscuits, 5 oz.	17.60	1.50	46.05	4.25	72.25	525
Nut food+1 oz Trumilk, 6 oz.	6.31	3.81	27.20	54.14	81.03	948
Trumilk, 25 oz.	0.06	0.41	1.74	1.99	2.90	35
Total 11½ oz.	23.97	5.72	75.09	60.38	156.18	1,508
SUPPER.						
Oatmeal, 2 oz.	4.03	0.88	12.94	5.34	33.02	236
Lard, 4½ oz.	.....	.....	.....	127.57	.....	1,187
Sugar, ½ oz.	.....	.....	.....	.....	14.17	58
Beef powder, 1½ oz.	0.99	3.85	35.29	2.37	.....	166
Glidine, ½ oz.	1.28	0.08	11.78	0.14	0.82	53
Biscuits, 1 oz.	3.52	0.30	9.21	0.85	10.85	105
Trumilk, 1 oz.	0.13	1.66	6.70	8.00	11.58	150
Sugar (additional) lump, ½ oz.	.....	.....	.....	.....	14.17	58
Total 11½ oz.	9.95	6.77	75.92	144.27	126.55	2,139

Meat extract, ½ ounce issued with supper ration.
Tea, ¼ ounce, issued with lunch ration.
Lime juice, ½ ounce, Cerebos salt 1/8 ounce daily.
Total Calories per day=5,512.
Total fat per day=320.58 grammes.
Total protein per day=226.93 grammes.
Total carbohydrates per day=409.28 grammes.

The Largest Centrifugal Pump

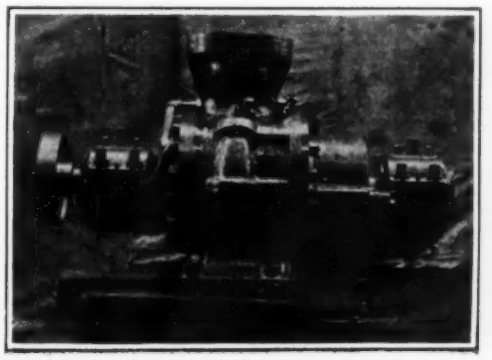
MODERN engineering enterprises are, wherever possible, conducted on a gigantic scale, for it has been found here, as in business, that doing things by wholesale is cheaper than retail methods. A recent case illustrating this occurred in New Orleans, where very large quantities of water had to be handled in drainage work, and to meet the requirements four immense centrifugal pumps were ordered, which are said to be the largest of their kind in the world.

These big pumps have a delivery opening of 78 inches diameter, and are capable of delivering 168,000 gallons of water per minute at a one foot head, which is equal to the flow of a good sized stream. The head of water will vary under different working conditions from one to thirteen feet, and at this latter difference of level 90,000 gallons per minute can be handled, but the pumps are required to turn up much faster than at low lifts, for whereas they do their work at 55 revolutions per minute when the head is but one foot, it requires 113 revolutions to move the stated amount at the 13-foot head. This, however, is by no means excessive, and the pumps will be very economically operated by Nordberg-Corliss compound condensing engines.

Besides these monsters there will be a fifth pump in the outfit which has an outlet of 48 inches, and a capacity of 47,500 gallons per minute at 3-foot head, as against 156,000 gallons at the corresponding lift by its big brothers. These pumps are built by the Southwark Foundry and Machine Company of Philadelphia.

A picture of one of the big 78-inch pumps, as it appeared in the erecting shops of its builders, is shown in the cover illustration of this issue, and some impression of its unusual proportions can be obtained by comparison with the figure of the man who stands in the outlet opening.

As a contrast to these huge monsters a small high-pressure, three-stage turbine pump by the same makers is also illustrated, and this is a little giant in its own way. The outlet of this small pump is but 6 inches in diameter, but when running at 3,400 revolutions it delivers 750 gallons of water per minute under a head of 550 pounds, which is claimed to be the highest head attained by any pump of its kind yet built in this country.



The little giant high-pressure pump.

Maps and Map Making*

An Historical Review of the Development of the Art of Topography

By Sir Thomas H. Holdich

A SAVAGE often possesses the instinct of rudimentary map-making; that is to say, he has the fundamental instinct in him which certain animals (I might say all wild animals) have for direction. I remember once searching for an old and disused salt cave on the Persian coast which was said to be inhabited by devils and other unpleasant inhabitants, and which possessed extraordinary subterranean scenery of great wonder and beauty. It was a very hot and a very weary quest. We should, I think, have abandoned the search among the low hills which crowded down to the shore line, but that we observed a ragged and scantily clothed individual sitting in solitude on the shore with a suspicious lump of something before him which he was guarding carefully. The practised eye of my companion observed a native craft on the blue horizon, which he rightly conjectured to be engaged in illicit salt traffic, and we at once found a way out of our trouble. The man on the shore could speak no sort of dialect that either of us knew, and we could only point helplessly to the lump of salt and to the hills, and insinuate by means of certain coins that we were not there to interfere with him, we only wanted to see where that salt came from. So with his fingers he drew a map on the sand, and he did it so well that half an hour's climbing led us into the narrow mouth of the cave, and we saw there what I believe no other European had ever seen.

The point of the story lies in the fact that precisely the same symbolism, the alphabet of map-making, came as naturally to the hand of that smuggler as it has to the hand of all map-makers since the days of Ptolemy till now. The very earliest maps of which there is any record are those of Claudius Ptolemeus, which were made about 100 to 150 A. D.; but the earliest copies of them to which we can refer did not exist till about the year 1400, thirteen centuries after they were actually compiled.

But there were other maps during the thirteen centuries which elapsed between the making and the publishing of Ptolemy's maps, and some of them are remarkably interesting. St. Jerome's translation of the Greek work, "Onomasticon," A. D. 388, deals with Biblical countries. A famous map of the world was compiled by a Spanish monk, St. Beatus, in A. D. 776. This makes the earth square, plants the Garden of Eden at the top of the map (which is the eastern side of it), with Eve and the serpent in orthodox relationship. The next important map is the "Cotton," or Anglo-Saxon, map of the world, dating from about the end of the tenth century. Here Jerusalem is no longer the center, and Taprobana, or Ceylon, is at the top. A map dating from 1200 again places Jerusalem in the center, but from the Garden of Eden at the top there flow out five rivers, the Ganges being included. This is a diminutive map designed to illustrate a psalter, but room is found in it for a great variety of human monstrosities such as are common to all mediæval maps. These remarkable additions to mediæval mapping are really interesting as indications of the nature of the ethnographical knowledge of the time. It is quite probable that they represent the prevailing ideas of the educated, i. e., of the priestly, class regarding the strange living world beyond their immediate ken. About the year 1350 and for a century later there was a popular history in England written by a monk at Chester called the "Polychronicon," and in it there are two maps compiled in the conventional style of the day full of Biblical legends and popular myths.

Coupled with much information of ethnological interest about men with one eye or with hairy bodies, we find these curious facts stated for the enlightenment of posterity. Germany, it is said, has a greater population than it is able to feed, while Ireland is both larger and wider than England; it seeks liberty but *shirks work*. One of the latest of the mediæval manuscript maps in the British Museum is a copy of a map compiled in 1459 by Fra Mauro. "Its geography," says Mr. Villiers, "shows an immense improvement on the last map of 1350, Europe being especially good."

Of the earliest printed maps just one or two require a short reference. There is the map known as the "Cusa" map, compiled before 1464, which is printed from a copper-plate by Cardinal Nicolas of Kuesan, which was the chief map of its time; and a map of the world in the "Insularium illustratum" of Henricus Marletus Germanus is to be noted as the first showing Portuguese discoveries. At this point in map history commence the first rudiments of modern cartography. A Dutch printed map by Johannes Ruysch first represents India as a peninsula. That was in 1507, and it indicates the gradual progress of geographical knowledge. The Waldseemuller map of about the same date introduces the New World and further defines Portuguese discoveries; and yet another Dutch map (for the Dutch may claim to be pioneers in this branch of the art of printing), usually known as Jan Severseen's map, which undoubtedly is much indebted to the Waldseemuller map for its material, is quite a good example of early topographical art. The mountains now assume the appearance of lines of haycocks, and the trees indicating forests are all of the pointed Noah's Ark variety. Strange beasts, including an enormous elephant, stray over the continents, and monsters of the deep indicate the perils of the sea. Thenceforward the gradual evolution of the printed map passed into the hands of Italians, who took a much stronger lead in geographical illustration than in geographical discovery. Gerard Mercator, at the end of the sixteenth century, was the great map-maker of his day, and his projection (notable, perhaps, for its simplicity) is in use at the present time. Mercator gives us by far the best map of Ireland yet produced.

It is to be regretted that the greatest geographers of the mediæval ages (the Arabs) have left so little in the shape of cartographical illustration behind them.

There is no doubt a feature common to all these old-world maps which cannot be admitted now, but the old-world endeavor to unite ethnographical knowledge to that of more prosaic geography is no longer considered admissible. In fact, it is the extraordinary wealth of an infinite variety of information conveyed by legend and by pictorial illustration which renders the old form of mapping so interesting and so confusing. There are indeed certain details which may be considered as historically useful. We learn something of what our forefathers knew about the world they lived in. We learn what their shipping was like, even if we cannot admit their sea-born monstrosities. We have found that in some details of geographical information they possessed a knowledge of facts which have been discarded since their time, only to be revived again as correct in modern days, as, for instance, in the case of the Nile sources and the mountains of Central Africa.

With the gradual evolution of a system of cartography which should first of all divide land spaces from water by outlining the continents and islands of the globe and thereafter separate mountain systems from plains and secure some representation of the hydrography of the earth by delineating the course of a great rivers, there arose the imperative necessity for a form of projection of the map, the definition of parallels of latitude and meridians of longitude, by means of which the true relationship of separate areas of the earth could be correctly maintained. Obviously this necessitated careful measurement of the earth's surface in order to determine its figure and to establish laws and formulæ whereby these parallels and meridians could be laid down accurately previous to the introduction of topography. Thus arose the science of geodesy, or earth measurement.

It cannot be too strongly insisted on that all sound map-making must be based on sound geodetic measurement in the first instance. To put it shortly, Triangulation, which is the outcome of geodesy, is called upon to supply the framework or skeleton onto which the topograph or the body of the map has to be filled. This framework is furnished with a projected graticule system (i. e., with lines of latitude and longitude correctly defined), so that the picture or plan of the country in question can be carefully adjusted by means of the points fixed by the triangulation, which are, of course, common to both the geodetic projection and the topographical map. This adjustment is the work of the cartographer. Sometimes, as in India, the cartographer and the actual topographical surveyor in the field are one and the same person. Sometimes, as in Russia, a highly practised cartographical artist is employed to take the field sheets from the hand of the topographer and translate them to the finished map without reference to the surveyor at all. It is this latter method, doubtless, which produces the most artistic effects, but it should be remembered that topography is not only an art, but a twofold art. There is the cartographic art of translating the field map to the final sheet for publication, and there is the art, only really given to few, of representing Nature on the field-map as she actually exists. The art of the topographer thus combines that of the painter and the engraver.

I need hardly say that when I talk of making map pictures from Nature I am talking of the plane-table method of making them. It is the one process which fully deserves the appellation of art, and the beauty and advantage of this system of map-making is that it appeals to the highest artistic instincts. I do not mean that an inartistic effort may be inaccurate or useless.

If accuracy is our object, then no professional cartographer, however gifted with artistic imagination and draftsman's skill, can take the place of the man who sees and knows, into whose eyes the picture of great world-spaces has entered. Unfortunately, gifts of imagination combined with draftsman's skill often make very pretty showing. Experience will enable a clever cartographer, a draftsman who has never been in the field, to represent even complicated topographical features of an imaginary character with a skill such that even an experienced surveyor can hardly detect the lies in it; and so it comes about that if a map is to be judged by its accuracy and faithfulness to Nature, apart from artistic effect, it is very often the most unattractive maps that are the best. Please remember this, and remember also that the processes of reproduction vary infinitely, and that when you look at a really beautiful production, full of harmonious light and shade and delicate indications of graceful features where effect requires them, and exclaim "What a lovely map!" you may be merely the innocent victim of a skilful cartographer who has been backed up and perfected by a first-rate publisher.

It is interesting to observe the slow but gradual development of the art of topography from the earliest maps until now. The alphabet of the process has remained much the same. Mountains and plains, rivers and coast-lines have been simplified and perfected so as to produce the most readable map, but the alphabet has not fundamentally altered. It is in the matter of relief chiefly that the change is noticeable. The earliest attempts at depicting mountains by pictorial elevation have given place to plan. The little groups of conical excrescences, like bands of haycocks, were by no means unintelligible, though purely conventional. Indeed, it may be doubted whether to some intelligences they are not even now a more effective illustration of hilly country than our latest and most scientific system of contouring. Between the two we may find every variety of method employed which can produce the effect required. In large scale maps dealing with comparatively small areas and involving no great differentiation in altitude it has long been decided that the only scientific method of depicting the country is by lines of contours (i. e., line of equal level) at definite vertical intervals. It undoubtedly has its drawbacks. There may even be a difficulty here and there in the distinguishing between an elevation or a depression unless the value of the contours above the datum are distinctly stated in figures. But it is beyond dispute that for all military or fiscal purposes the contoured map is the only one admissible.

Difficulties arise when the elevations on the face of the map rise from the status of hills to that of mountains, and with the gradual steepening of slopes the delineation of contours gets closer and closer until it is impossible to introduce them on small scale maps without overlap and confusion. This applies to the many forms of geographical maps from the scale of one inch to the mile downward, when it usually happens that some more artificial and less conventional method of illustrating mountains in plan is necessary. The difficulty is usually met by "hachuring," that is to say, by introducing broken instead of continuous contours, and abandoning regularity of vertical interval. It is here that the artist steps in.

The weakest link in that chain of processes which is the bases of scientific map-making lies in the process of combining new work with old. People ignorant of the processes of map-making can have little idea how much the process of revision enters into the matter. The rapidity with which fresh artificial geography in the shape of roads, railways, houses, etc., springs into existence, combined with the action of wind and weather in changing the face of Nature, taxes all the resources of the Ordnance Survey to cope with it. And it is almost impossible to maintain a high standard of up-to-date mapping, which is applicable to any one map in a sheet of any size, all over the face of it. But when we come to the class of mapping which

* Extracts from inaugural address before the Royal Society of Arts.

we include under the term geographical, i. e., wide areas depicted topographically on small scales within the rigid lines of a restricted sheet, these variations in quality of accuracy become far more pronounced and more important. No one but the compiler (who keeps the record) can possibly say what is the exact value of any one portion of the map, how far it is to be trusted in the business of conducting military affairs or the international arrangement of boundary settlements.

A naive belief in the accuracy of a printed map compilation has foundered many a high political programme, even when the geographical features which may form the basis for a treaty have already been correctly shown as they existed at the time the map was made. The face of the earth changes and locally changes very fast.

No need to elaborate this propensity of our earth world to change its face or of its more primitive inhabitants to change their habitations and call them perpetually by new names. I only refer to it as a caution to map cities.

I have said a word or two indicating that the gift of imagination must still have its place in map-making. This may sound almost immoral, although it is but a page from past records, and yet I have come to the conclusion that if the public is asked to exercise imagination in reading the map, the map-maker must meet the public half way. It is still, of course, in the domain of geographical mapping where the conformation of the ground is partly well known, partly indifferently known, that imagination must here and there be called upon to supplement actual knowledge. If the map-maker were to confine himself to absolutely what he knew to be true in the delineation of mountain and valley, or of rough desert areas wherein no living creature moved, he would simply leave large white blanks in his map which might in themselves be most misleading.

It used to be the fashion to leave such blanks, but in my opinion it is better to introduce mountains or deserts where they are known to exist, to indicate all that can be seen from a distance than to make no sign at all. The part of the map which is conjectural should, of course, be carefully indicated; but there is doubtless a tendency on the part of enthusiastic explorers to consider much of these conjectures as certainty and to make no specific distinctions between what they know and what they think they know. In this way it often happens that succeeding explorers in the same region are apt to condemn the work of their predecessors from want of knowledge of the exact nature of the different parts of that work.

I may say just a word here about map criticism. It is a most common thing for even intelligent travelers passing over a country with a map in their hands to report that they found the map all wrong. Now, in the case of expeditions into partially known lands, it is only the expert who can say whether the criticism is a just one or not. It most cases it is not, for the simple reason that ordinary travelers have no means of identifying their position and do not know exactly where they stand; while, as regards place names, although they may succeed in identifying the position named, they are at the mercy of native guides and interpreters for their own information.

There is one matter about which experts disagree more or less, and it is one on which I think the public might well express an opinion. It is the fashion now to indicate successive planes of elevation by flat tint, and the ordinary application of color for that purpose is in shades of green for the lower elevations, rising into brown which deepens with the altitude. So well known is this system that I think it may be considered to have established a fair claim to permanence, so far as British maps are concerned. For my own part, I can imagine nothing better for ordinary touring maps. But difficulty arises when the altitudes are great, rising to many thousands of feet, as in mountainous regions. Here it is admitted that no one universal system of color-printing is satisfactory. This is not an unimportant matter, for it directly affects geographical education, which now is so largely assisted in schools by wall maps. In the effective preparation of these we want the opinion of both teachers and of artists.

Astronomy in the Arctic*

Some of the Duties and Hardships of Observers in the Frigid North

By Russell W. Porter

Some twenty years ago it was the fortune of the writer—or misfortune, rather—to become inoculated with the virus of that strange "wander-lust" known as the "Arctic fever." Since that time he has taken ten voyages above the circle. The following notes on the part of his labors bearing on the astronomical problems arising in the extreme North may be of interest to amateur observers who have never strayed so far afield.

The duties of the astronomer of a polar expedition are the determinations of the latitude and longitude of the winter quarters, and Greenwich time. There are other activities, such as the mapping of any new lands, observations in connection with magnetic work, etc.; but the first-mentioned are the most important, particularly a knowledge of the Greenwich time—for, as we all know, the meridians of longitude converge at the Pole to a point, and the longitude is the difference between local and Greenwich time. Therefore, it is quite necessary that a person traveling toward the Pole over a shifting sea of ice should know upon what meridian he is journeying, and that on his return from the Pole he should know upon which of the meridians he is descending, in order to arrive safely at his base of supplies. The compass, due to its weak horizontal component, is too unreliable a guide.

The Greenwich time is transported from civilization to the winter quarters by means of chronometers on the ship. Were their rate of gain or loss uniform, this method would be sufficient. But the shock of the ship in forcing a passage through the ice is very great, and the clocks change their rates, so that, arriving at winter quarters after a summer's battering through ice-floes, a knowledge of the time at Greenwich is very poor and not to be depended on. So recourse is had to other and absolute methods—either by occultations of stars by the moon, or moon-star culminations. From either of these observations the center of the moon is determined in right ascension at a certain instant of local time, and by entering an almanac and finding what the Greenwich time is when the moon has this right ascension, and by taking the difference between these two times the longitude is at once obtained. The degree of accuracy so found is not great; but it must be remembered that a degree of longitude near the pole is a very much smaller quantity than at the equator, so that, for practical purposes, quite a large error is admissible.

On the writer's last Polar expedition, an observatory was erected on a basalt ledge one hundred yards from the house. It was about 7 feet square, with walls of matched sheathing, north and south shutters, and a shutter in the flat and slightly inclined roof. A transit instrument by Repsold with 12-inch circles reading to single seconds of arc was set up on a brick pier. The chronometers at the house were never disturbed. Beats from the sidereal break-circuit clock were heard by means of a sounder on the wall of the observatory. A collimating pier a few feet north of the transit served for the north meridian mark, while a signal made of several biscuit-tins, one on another, placed on a glacier a few miles away, served as a south meridian mark. By swinging the instrument out of the meridian, and sighting through a hole in the wall, the magnetometer of the magnetic observatory some thousand feet away could be seen, and thus all magnetic declinations referred to the primary meridian.

The writer spent many long hours in the observatory through the winter night with his eye to the telescope, watching the tiny star disks passing across the seven vertical threads of the reticule. Dressed in heavy furs, with one hand bare to record the time-intervals, he was obliged to sit motionless for sometimes two or three hours at temperatures ranging from 30 degrees to 60 degrees below zero, with no artificial heat, and the bitter wind passing freely through the building. At times the room would suddenly fill with a fog so thick as to require an immediate cessation of work. Often the oil in the illuminating lamp hardened to the consistency of slush, and the lamp refused to burn. Tiny ice particles of hoar frost were very bothersome, settling on the pivots of the transit, and throwing the horizontal axis out of level. There was no running around the block, stamping the feet, or swinging the arms to restore circulation—just a steady, nerve-racking watch.

Those observing at the magnetic hut fared no better. Here the vagaries of the needle were watched, and recorded every two minutes, over an unbroken interval of nearly two years.

The peculiar conditions surrounding the pole present other obstacles. The sun, never mounting high in the heavens, is weak in actinic rays; hence the photographer has his troubles. In using an artificial horizon with a sextant, the small angle of incidence prevents the whole disk of the sun from being observed; in fact, the small transit instrument is altogether preferable to the sextant in polar work. Those made especially for the Ziegler polar expedition were but little heavier than the sextant and mercury basin, and far more useful.

During the long summer's day of four months the writer has tried to find the moon, but has never succeeded. The heavens are not blue, but a bright, glaring grey, due to the reflection of the sunlight from the surface of the snow. It very effectually "puts the moon out of business," as far as the astronomer is concerned. On the other hand, in the winter night the moon is above the horizon continually through half of each lunation, and has much the same appearance as in lower latitudes.

Imagine yourself at the pole, with the meridians of longitude radiating out from your feet until they disappear over the rim of the horizon. It is about June 21st, the summer solstice, and the sun is revolving around the heavens at an altitude of some 23 degrees, as ever just once in twenty-four hours, but neither rising nor descending in its path through the sky. What time is it for you as you stand there—that is, time as understood by an astronomer? The answer is easy: there is no local time. It is equally true to say that it is all hours. Now step off the pole toward the sun, wherever it may happen to be. You are now on the meridian over which the sun is situated. It is noon to you—astronomical noon. Take two steps backward: you have crossed the pole, are on the same meridian, and it is astronomical midnight. In this manner you can make the time of day anything you choose by simply taking one step away from the pole in the direction of the meridian on which that particular time obtains.

If you could afford to remain at this unique spot throughout a year, you would see the sun start on a slowly descending spiral until at the autumnal equinox he would be rolling around the horizon like a big billiard ball. The next day he would show only his top edge, and the next would disappear entirely for six months, slowly spiraling downward until at the winter solstice he is 23 degrees below the horizon, and begins his slow return. In March he again puts in an appearance, repeating his performance of the fall before, but in reverse order, until on June 21st he is at his highest. These would be the phenomena, provided you could see them; but the chances are that there would be but fugitive glimpses now and then, as the snow is drifting almost continually in high latitudes.

The auroral displays are, of course, of common occurrence beyond the circle, and at times in the winter become so brilliant as to lighten up the "snowscape" like full moonlight. In Franz Josef Land, north of Russia, the streamers predominate, the auroral arch not being common; but in the vicinity of the north magnetic pole the arch is seen more often. The writer has never been able to hear the peculiar sound resembling rustling silk so often spoken of by Arctic explorers as accompanying a brilliant display. And it is his impression that the great magnetic disturbances as observed in the magnetometer in winter are not necessarily accompanied by exceptionally bright aurorae.

* From an address delivered before the First International Conference of the Society for Practical Astronomy, Port Clyde, Maine.

Where Timber is Wasted

It will surprise most people to learn that boxmakers in the United States use more than four and a half billion board feet of lumber each year, or more than one tenth of the entire lumber cut of the country. This is the authoritative statement made by the Forest Service of the Department of Agriculture, and although most of these boxes are used several times, still it is suggestive of a very considerable portion of our fast disappearing forests. Already the high price of suitable lumber is forcing manufacturers to seek other materials and more economical methods for packing and shipping their goods, but there is room for much greater developments in this direction.

Over the ocean by rail.

To Cuba by Rail

The Last Link Between Key West and Havana Joined

By Harry Chapin Plummer

To Cuba by rail! The dream so cherished by Henry M. Flagler, the founder of the Florida East Coast Railway system, has found its ultimate realization in a wonderful car-ferry steamer, stated to be the largest and most capacious of her type in the world. This powerful craft, which bears Flagler's name, and which is designed specially for the 100-mile run between Key West, Fla., and Havana, Cuba, will complete the final link in the passage of freight, without trans-shipment, between the cities of the United States and Havana, Santiago and interior points on the island of Cuba.

At the Havana terminus of the ferry, the steamer

Map of the Florida-Havana route.

effects a track connection with the United Railways of Cuba, which, like the trackage aboard the ferry-steamer and that of the Florida East Coast Railway, are of standard gage.

In their journey from the north to Key West, trains pass over the $15,000,000 viaduct system that bridges the 107 miles of alternating stretches of water and coral islets lying between the mainland of Florida and Key West. The car-ferry steamer "Henry M. Flagler," which goes into service next month, will fill the last gap in the journey to Havana and, so, fulfill the purpose of the builder of the wonderful chain of viaducts.

It is in the movement northward from Cuba of grape-fruit, oranges, bananas, pineapples, and other products of the "Pearl of the Antilles," which are liable to the greatest danger of ruin or injury when transfers are made, that the new vessel must prove her worth at the outset. On her southward runs from Key West she will move trains largely laden with dressed beef and foodstuffs originating in the West and the North of this country.

Equipped with four sets of tracks of standard gage on the car deck, which is 360 feet long and provides accommodation for thirty of the largest size refrigerator cars, the ferry-steamer is, in addition, fitted with three cargo holds. These can be loaded either direct from the cars or through cargo ports at the side of the vessel.

The dry cargo capacity of the boat is approximately 3,000 tons, with an additional provision in one of the forward ballast tanks for the shipment of a bulk quantity of molasses. Each cargo hold is served by an independent double-drum electric cargo hoist of two tons capacity, and the necessary filling and discharging apparatus has been fitted to the tank intended for molasses.

Ten water-tight compartments, into which the vessel is sub-divided, are used for ballast purposes. These deep tanks have a capacity of 3,000 tons, and the steamer thereby can be brought down to her proper draft when no cargo is carried. A system of pipes connects the tanks and for emptying them two 12-inch centrifugal pumps have been fitted, which have a capacity capable of clearing all the tanks within an hour and a quarter.

Two triple-expansion engines of the usual marine type, having cylinder 20-32½-34 inches diameter by 36 inches stroke, compose the power plant of the vessel, each developing 1,500 indicated horse-power at 100 revolutions per minute when operating under 170 pounds steam pressure. The voyage of 100 miles (from dock to dock) is to be made in eight hours—at an average speed of 12½ miles per hour.

The boiler plant will consist of a battery of four Scotch type boilers, each 13 feet 2 inches in diameter by 12 feet long, and each fitted with two 48-inch Morrison corrugated steel furnaces. The boilers will be operated under the "Howden" system of forced draft, and they are built for a working pressure of 170 pounds. The auxiliaries are more than ordinarily complete, the feed, circulating and ballast pumps being duplicated, as is also the electric plant.

A notable feature of the boat is the fine accommodations provided for the officers and crew. Four individual baths are provided for the officers, while the crew's quarters are equipped with four shower-baths. Running hot and cold water is furnished for each room, and cold salt water for the shower and bath-tubs.

The steamer, the keel of which was laid April 20th last, was built at the Cramp Shipbuilding Yards, Philadelphia, and launched September 22nd. M. C. Furstenau, consulting naval architect for the Florida East Coast Railway, designed the plans and specifications.

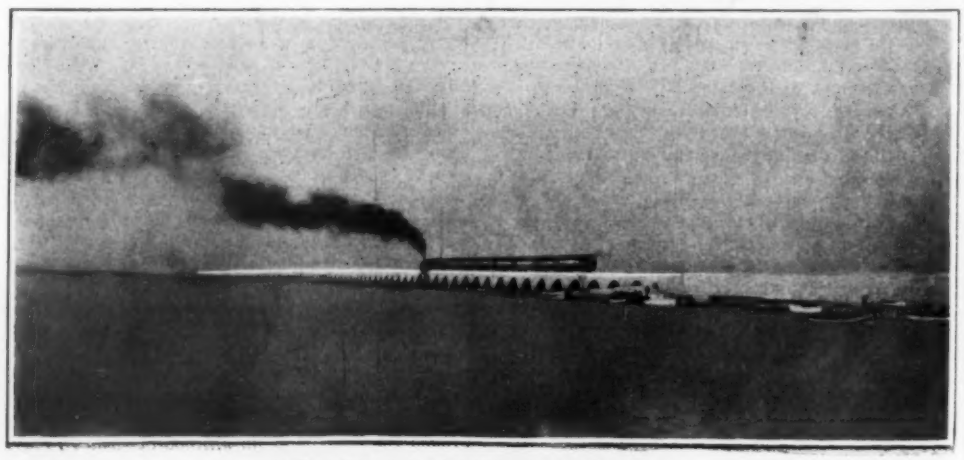
The idea of quick communication with Cuba is not new, for the promoters of the Florida East Coast Railway have had it in view for many years, but the accomplishment has been slow on account of the many difficulties to be overcome. It was no small undertaking to extend the line down the sandy, marshy shore of Florida to Miami, where a long pause was made. Then came that daring engineering feat of carrying a railroad over the swampy tip of Florida, and thence by a remarkable series of embankments and concrete viaducts, using the long curving series of keys as stepping stones, to Key West, the outermost accessible point that can be reached in this manner.

In building this wonderful viaduct, which is 128 miles long from Homestead at the tip of Florida to Key West, earth and rock embankments were used wherever the depth of the water permitted, and over fifty miles of this kind of roadbed was constructed; but in many of the intervals between the keys, of which about thirty were linked together by the undertaking, the water was too deep to permit of filling in, and moreover, many of these openings were navigable passages that could not be closed or were in such exposed positions that embankments would not stand. Here a series of massive reinforced concrete viaducts were placed, being built with arches of 50 or 60 feet spans. Altogether these viaducts have a total length of somewhat over five and three fourth miles, there being four separate viaducts, running from 4,950 to 10,500 feet in length.

At the time the road reached Key West it built a substantial drydock, together with ample wharves, so that it was all ready when the time came to establish the hundred-mile ferry to Havana, as has now been done.

Electrolytic Silver-cleaning Method

This is an original method which is the object of a French patent. Instead of cleaning solid silver or plated objects by the usual powder polishing, which, of course, has a bad effect on the surface, such objects are simply put in a non-metallic basin (enameled ware or the like) and are cleaned by electrolytic action alone. This is done by putting on the bottom of the basin an "electra" plate of specially prepared metal, the bath being a solution of carbonate of soda. From three to five minutes in the bath is all that is needed in order to remove all oxidation from the surface of the silver objects, then rinsing and drying. According to a statement by the Paris Municipal Laboratory, there can be no deterioration of the objects by this process.



The viaduct that links Florida to Key West.

The car-ferry steamship that transports trains from Key West to Havana.

The trip from Havana to Key West will be made in eight hours, and perishable cargoes, such as oranges, bananas, and pineapples, loaded in cars, will be sent through to any American city without trans-shipment or handling.

Building the great Key West-Cuba car-ferry steamship. Showing the opening in the stern where the trains are run on board.

Four tracks for trains are laid on the main deck, and besides carrying thirty of the largest refrigerator cars, the vessel can carry a large quantity of molasses in bulk in her tanks, as well as much freight.

Apparatus for Demonstrating Newton's Laws

Studying Motion Accelerated Under the Action of a Constant Force

By H. W. Harmon, Grove City College, Grove City, Pa.

Most of the standard text-books in physics and mechanics, under the topic of Newton's Second Law of Motion, give more or less space to the discussion and solution of problems concerning the tensions in cords attached to accelerated bodies, using the cables attached to rapidly ascending and descending elevators for their illustration.

Several years ago it seemed desirable to design an apparatus suitable for our students in the physical laboratory, to test out these principles experimentally. The difficulty met with at once was to find a practical method of measuring these tensions while the bodies were in such rapid motion. Finally, the apparatus shown in the diagram was developed and has been in use by our students for the last two or three years.

As inertia, acceleration, changes of momentum, and reaction are all involved in the experiment, it goes with us under the name of the Newton's Laws Experiment.

Referring to the diagram, Figs. 1 and 2 show a top and front view of the apparatus. The body to be accelerated is a car shown in Figs. 3 and 4, arranged to be variously loaded. It slides over the surface of a hard wood box about 18 feet long and 2-inch by 4-inch section. The car is released from its starting position by the action of the electromagnet (H) shown in Fig. 3, and operated by the clock pendulum, Fig. 2, making and breaking contact. The car is accelerated by the unbalanced weight of the mass M_1, which may also accelerate M_2 if it is attached to the rear of the car, as shown in Fig. 2. The cord connecting the car and M_1 passes over a frictionless pulley mounted on the movable arm shown in Fig. 5. It is apparent by a glance at the spring balance (B) in Fig. 2, that the tension (T_1) in the cord connecting M_1 and the car will be registered by balance B, if allowance be made for the weight of the arm and a slight leverage action of the pulley itself. By this simple scheme we were enabled to measure the tensions in the cord attached to the accelerated body regardless of the high velocities attained.

A curve card is plotted in which the weight of the arm and the leverage of the pulley is allowed for, and by its use, balance readings can be taken off from the curve as true tensions (T_1).

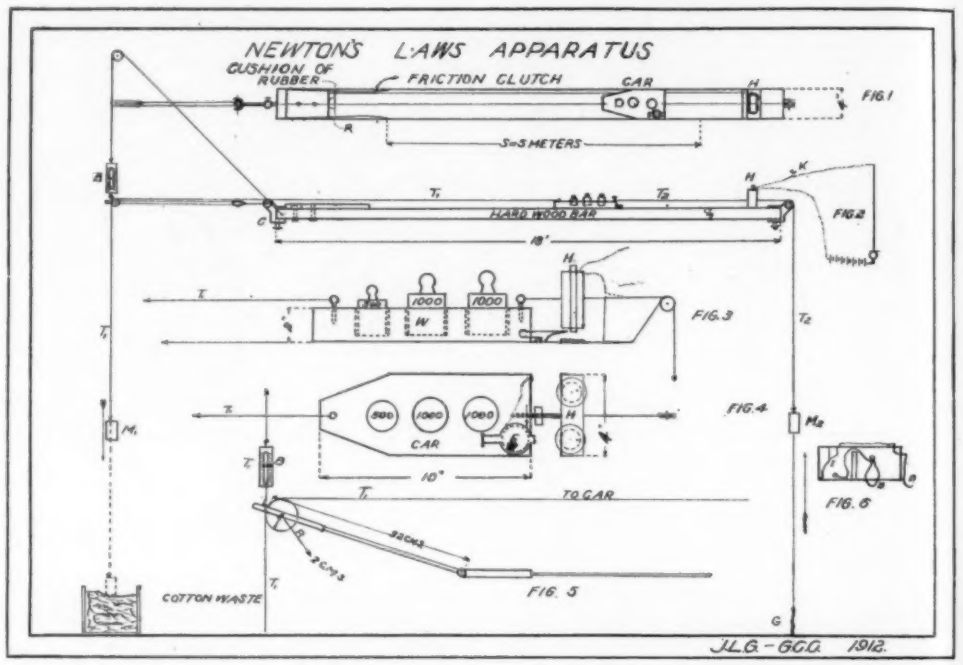
This balance conection is made as follows: With the pulley arm in the horizontal position and the tension T_1 zero, the balance was found to read 125 grammes, owing to the weight of the arm. This weight is included in all cases in the reading of B. But more or less completely neutralizing this weight of the pulley arm is an upward component of the tension ($= 2/32\ T_1$), for as used in this position the pulley, the pulley wheel and arm constitute a right-angled, bent arm lever with power arm equal to the radius of the pulley (2 centimeters), and the weight arm equal to the length of the pulley arm (32 centimeters). With this correction for the weight of the beam, the equation connecting the balance reading B with the tension T_1 will then be:

$$B = 125 + T_1 - \tfrac{1}{16}\ T_1.$$

To draw the correction curve, remember that when $T_1 = 0$, then $B = 125$ grammes; when $T_1 = 125g$, $B = T_1 = 2{,}000$ grammes. Locate these two points on a sheet of cross-section paper by the usual method and draw a straight line through them. (See balance correction curve diagram, Fig. 2.)

To keep the car on the track, the bar has a half round groove formed in it through its entire length, and a corresponding half round runner is fastened to the bottom of the car. To keep the friction constant, the sliding surfaces are kept uniformly polished and oiled with heavy oil. The high velocities attained, 5 meters per second or more in some trials, are reduced without too much shock by the friction clutch and rubber cushion shown in Fig. 1. The velocity of M_2, when attached, is reduced to zero by a spring or gum tube (G) attached to a trailing cord which goes into operation at the same time the friction clutch does on the car, while M_1 is brought to rest in the box of cotton waste shown in Fig. 2.

When the apparatus was first put into practical use the car was released by a trigger operated by hand and the timing done with a stop-watch. Thus operated, the errors due to timing gave results in which the percentage of error was in some trials quite large. Recently we have mounted on the car an electromagnet (E) shown in Fig. 4, which is arranged in series with the releasing electromagnet (H). The armature of this magnet, pivoted to move up and down, has a spring brass wire soldered to it, extending over the back end of the car and bent down near the surface of the sliding track, where it terminates in a piece of chamois skin moistened with well thinned printer's ink. This is shown in Fig. 6 by I. The electric current is led to the car by two wires, one stretched taut down the back side of the bar; the second one is stretched down a second half round groove in the top of the bar. Contact brushes BB for these are shown in Fig. 6. The inker marks off on the bar distances covered in even seconds as beaten off by the seconds pendulum of the laboratory clock. The slotted weights which go to make up M_1 are so chosen that the unbalanced force in the various trials of the experiment causes the car to make its run down to the clutch in just slightly over 2 seconds.



The inker operates very accurately, for, even in those trials in which the car is traveling 500 centimeters per second, successive trials seldom differ by more than two or three centimeters in the total distance covered in the 2 seconds' time of run used. This indicates that the combined error of the inker and release magnet does not exceed 1/200th of a second in the timing. Since equipped with this improved timing device, the apparatus has given very satisfactory results, as can be seen by an examination of the data table. The errors remaining are almost entirely due to balance readings and friction measurements.

The friction is found for each trial as the car is differently loaded. This is done by placing on the weight support, M_1, such slotted weights as will cause the car, when started, to move down the track with a slow uniform motion. In those trials when M_2 is attached to the rear of the car, the total of the slotted weights thus needed, less the weight of M_2, is called the friction.

Method and Theory of Experiment.

First clean, oil and polish the sliding surfaces, then load the car as required and find the friction (F_f) as above. The required weights are next placed on the M_1 weight holder and the car placed at its starting position and held by the trigger ready to be released, following the closing of the electric switch (k) in the clock circuit. When the car is released and is speeding down the track toward the friction clutch, one observer watches the index of balance B, and takes its reading. This is usually repeated three times, and the average reading of B is taken, corrected by reference to the curve card and recorded as T_1.

At the same time, for each of these runs, the distance the car has traveled in 2 seconds, as marked off by the inker, is measured, and these distances averaged ($= S$).

The unbalanced force (F_a), which causes the car and attached masses to be accelerated as it runs down the track, is the difference between the weight of M_1 and the friction (F_f) and also less M_2 if it is attached.

Newton's second law enables us to compute what this acceleration a will be. If absolute units are used, the equation is

$$F_a = Ma,$$

or, using the gramme-weight as the unit of force, and the gramme as the unit of mass,

$$F_a = \frac{Ma}{980}$$

where F_a is the unbalanced force, M is the total mass accelerated, and a the acceleration.

In our present problem the total mass M is given by

$$M = M_2 + M_1 + W,$$

where W is the mass of the car and load. The acceleration a as computed by the formula

$$a = \frac{980F_a}{M} = \frac{980F_a}{M_1 + M_2 + W}$$

(F_a being measured in grammes weight) should agree with a computed from

$$s = \frac{1}{2}at^2$$

$$a = \frac{2s}{t^2}$$

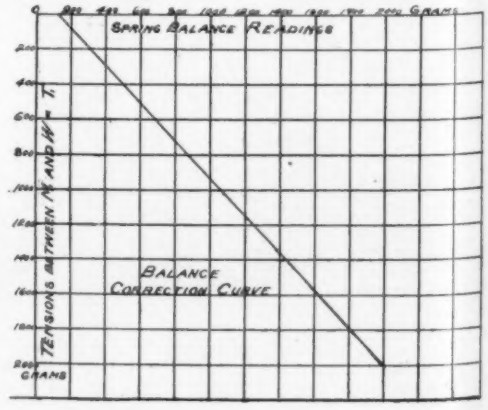
The tension T_1 in the cord connecting M_1 and W is equal to the sum of the frictional force F_f (measured in grammes weight) of the weight 980 M_2 of the mass M_2, if attached, and of the unbalanced accelerating force $F_a = \frac{(W + M_2)a}{980}$ grams. weight acting on W and M_2.

Thus $T_1 = F_f + M_2 + \frac{(W + M_2)a}{980}$

This should agree with the corrected balance reading T_1.

It will be noticed that the tension T_2 in the cord connecting the car with the mass M_2 is not observed experimentally by a balance and movable arm as is the tension T_1 in the leading cord attached to M_1. This could be done in the same way, but it is not deemed necessary for this experimental test to be repeated.

When the car is at its starting position, held by the trigger, the mass M_1 hanging at rest at the end of the cord exerts a tension (T_1) in the cord that is exactly



equal to its weight. The weight of M_1 at rest is fully effective in producing tension. A freely falling body has no effective weight, for its weight force is all used in accelerating it. A falling body only slightly accelerated has a proportional part of its weight effective (i. e., a man in a descending elevator). This effective weight approaches zero as (a) approaches (g) in value when (g) equals an acceleration of 980 centimeters per second per second, or 32.16 feet per second per second; and the weight of a body approaches full effectiveness as (a) approaches zero. We shall therefore expect to find the values of T_1 always less than M_1, and T_2 always greater than M_2, for M_2 is accelerated upward, and T_2 consists of both the weight of M_2 and the force which is accelerating M_2. Reference to the data table shows these expectations realized.

In looking into the factors which enter into this experiment, inertia, momentum, impulse, friction, and tension, it is seen that Newton's Three Laws are all fully involved, and also the laws of accelerated motion. A knowledge of most of the terms used in mechanics is necessary. This therefore should make an especially effective experiment for the junior student in physics or the student making a special study in mechanics.

No.	Masses			Friction F_f	Coefficient of Friction $F_f \div W = C$	Space S	Time t	Acceleration			Tension between				Unbalanced Accelerating Force On		
											M_1 and W			W and M_2 $= M_2 + F_a = T_2$			
	+ Mass M_1	Load W	— Mass M_2					Observed $= \frac{2s}{t^2} = A$	Computed $= \frac{980F_a}{W+M_1+M_2} = A'$	Error.	Observed T_1	Computed $F_f + F_a + M_2 = T_1$	Error.		M_1+M_2+W $M_1-(M_2+F_f)$ $= F_a$	$\frac{W+M}{(W+M_2)a} = \frac{}{980} = F_a'$	$\frac{M_2}{M_2 a} = \frac{}{980} = F_a''$
	grms.	grms.	grms.	grms. weight		cm.	secs.	cms. per sec.	cms. per sec.	%	grms. weight	grms. weight	%	grms. weight	grms.	grms.	grms.
1	1,865	3,160	0	860	.272	391.0	2.0	195.5	195.6	0.0	1,440	1,491	3.4	.....	1,005	631	.....
2	1,365	2,360	0	655	.278	370	2.0	185.	186.7	1.7	1,070	1,101	2.9	.....	710	446	.....
3	1,115	1,860	0	500	.269	394.5	2.0	197.25	202.3	2.7	855	874	2.2	.....	615	374	.....
4	700	1,060	0	305	.288	422.1	2.0	211.	220	4.4	528	532	0.9	.....	395	228	.....
5	1,965	660	1,000	270	.409	371	2.0	185.5	187.7	1.2	1,555	1,584	1.8	1,189	695	314	189
6	2,315	1,060	1,000	395	.372	413	2.0	206.5	203	1.7	1,780	1,821	2.5	1,210	920	426	210
7	1,715	1,360	500	440	.323	417.6	2.0	208.8	212.2	1.6	1,295	1,336	3.0	607	775	396	107
8	2,215	2,160	500	620	.287	440	2.0	220.	220.2	0.1	1,680	1,717	2.1	612	1,095	597	112
9	1,815	2,360	200	620	.263	443.2	2.0	221.6	222.7	0.5	1,395	1,399	0.3	243	995	579	45
10	1,915	2,660	200	700	.264	416.0	2.0	208	208.2	0.1	1,497	1,506	0.6	243	1,015	606	42.5

Sudden Changes in the Form of Liquid Crystals*

By O. Lehmann

FORMERLY science assumed matter to be continuous, although porous; to be chemically as well as physically homogeneous; the molecular arrangement changing of course in case of anisotropy at the border line between differently oriented parts of a body, in case of crystals, which may, however, be defined as aggregates. For example a spherical crystal has been considered an aggregate; whereas, according to my findings, it may be an individual.

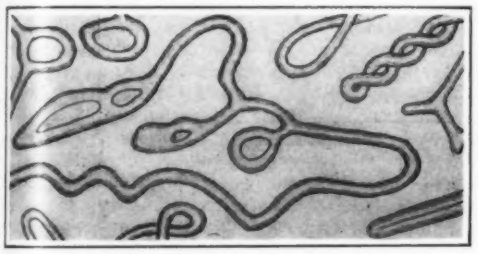


Fig. 1.

Likewise plastically distorted crystals (of gold, silver, etc.) were held to be aggregates of crystal fragments, while the existence of liquid crystals was wholly excluded from the realm of possibility by the "Theory of Identity" (molecular identity in the solid and liquid states), crystallization being made synonymous with solidification, only two sorts of molecular arrangement, the lattice-like order in the crystal and entire irregularity in the molten mass, being considered possible.

My discovery of transparent plastic crystals, whose distortion causes no fragment formation, together with the discovery that by the admixture of foreign substances permanent distortion of crystals can be secured, first justifies the question: "Are such distorted crystals still to be regarded as individual units, or are they aggregates as formerly supposed?" Must we distinguish between true crystalline plasticity, where the crystal remains a unit (for example, soft glass) and apparent plasticity, where a crystalline unit becomes an aggregate of fragments, the only sort of plasticity recognized up to the present?

The further discovery that the form of silver iodide stable above 146 degrees, formerly considered amorphous-viscous, was really crystalline, naturally raised the further question, "Can true plasticity go so far that a crystal may be considered fluid, thus contradicting the 'Theory of Identity?'"

I first obtained absolute proof of the existence of liquid crystals with ammonium-oleate hydrate. The proof rests on the demonstration that these crystals possess no "limit of elasticity," as the term is understood relative to solids; for the molecular equilibrium is automatically restored after any distortion; therefore, the crystals must be considered liquid, not solid.

When liquid crystalline ammonium-oleate is drawn into a capillary tube, the molecules arrange themselves radially, with optical axes perpendicular to the walls of the tube. Now, if a short piece of such a tube is placed in water the liquid crystalline mass swells out at either end into cylindrical myelin forms (liquid crystals), consisting of a modification of ammonium-oleate of identical structure with that in the tube.

Thus the existence of myelin forms is a complete contradiction to the above-mentioned assumption that a chemically homogeneous body must be physically homogeneous, for these myelin forms are not aggregates of crystal units, no boundaries being anywhere observable. Nevertheless, points near the surface are by no means equivalent to those near the axis; therefore *the matter is not continuous. The existence of liquid crystals, especially of the myelin sort, is then a certain proof of the molecular theory.*

The existence of liquid crystals also contradicts the accepted theory that only two forms of molecular arrangement can exist, the one entirely irregular, the other, the lattice-like order in crystals.

If, as I first supposed, the molecules are tiny rods, they must be radially arranged when sucked into a capillary tube; hence any current in the mass would alter the lines of interference, since internal friction would retard motion near the tube wall and give the molecules there situated an oblique direction. This does not occur, therefore I now assume molecules to be tiny flat plates, whose surfaces are perpendicular to the optical axis. Other phenomena support this assumption.

Accordingly a cylindrical myelin crystal is in a sense an aggregate of co-axial cylinders of regularly placed plate-like molecules, these cylinders being closed on the ends by hemispheres of like radius.

This peculiar structure demands anisotropy of thermic motion and of expansile force. The molecules easily glide over one another in a direction parallel to their flat surfaces, therefore expansion in the direction of the cylinder axis is greater than in the direction perpendicular to the axis; the *cylinder must then reach such a thickness that the greater capillary pressure of the hemispherical ends must exactly balance the excess of expansile force.*

It has not yet been possible to demonstrate thermic movements and their anisotropy by the observation of the Brownian movement, and since constant transitions from liquid to solid crystals occur it is very possible that the force hindering the surface tension from compressing the liquid crystal into a sphere (myelin forms) is not alone the force of expansion, but primarily the molecular directive force. A proof of this I believe I have discovered in the peculiar behavior of the myelin forms of ammonium-oleate and of protagon when cooled below —4 degrees.

AMMONIUM-OLEATE HYDRATE.

At ordinary temperatures the myelin liquid crystals of ammonium-oleate are jelly-like threads, easily bent, showing no elasticity, and no tendency to break even when bent double.

If liquid crystalline ammonia is placed under a large watch crystal shaped cover glass and ordinary ammonia allow to flow around and over the preparation, and the preparation set over night in a cool place, then such myelin forms as shown in Fig. 1 will be obtained. Now place these in a temperature of about —6 degrees and they will take on the zig-zag shapes of Fig. 2. We conclude that in the softer forms (Fig. 1) the molecules adjacent to one another in the direction of the long axis of a cylindrical surface form very blunt angles with one another; and that the straightening of the cylinders in the stable modification at the lower temperature is to be explained by the combination of the plate-like molecules into complicated molecules, which in turn are flat plates. The work done is a process of *direct transformation of chemical into mechanical energy.*

PROTAGON, PHRENOSIN, AND KERASIN.

Protagon behaves practically like ammonium-oleate hydrate. Phrenosins can be obtained in thin plate-like crystals. The liquid crystals obtained on heating these with water take the shape of cylindrical myelin crystals, resembling ray-like projections, and these on cooling suddenly, shrink suddenly to half their length.

If methelyn blue is added to the water in which phrenosin crystals are allowed to swell, so that the water is only tinged, the crystal becomes intensely blue

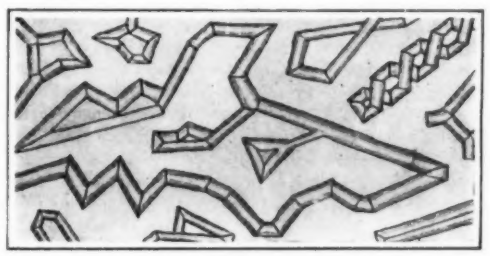


Fig. 2.

(probably through chemical combination). When on cooling the sudden shrinking above mentioned takes place the blue changes to red violet.

Kerasin acts much like phrenosin, expanding and contracting in the same way.

The myelin forms of protagon often lie one within another like the layers of an onion; forms open on one side drawing in, and ejecting smaller forms on solidifying and melting. They remind one of protoplasmic structures. Form and structure are, however, maintained purely by cohesion and molecular directive force, and not by an inclosing membrane.

* Translated from *Annalen der Physik* for the SCIENTIFIC AMERICAN SUPPLEMENT.

A Dog-Proof Fence

THE Department of Agriculture in a recent issue of its "Weekly News Letter" tells farmers how to build a dog-proof fence that will safely protect sheep from the ravages of these animals. The posts should be 7½ feet long, set 2½ feet into the ground, and spaced 16 feet apart. Along the surface of the ground stretch a barbed wire, this to discourage any attempts to crawl or burrow under. At three inches from the ground stretch 36-inch woven wire fencing having a 4-inch triangular mesh. Above this woven fencing, and at intervals, respectively, of 5, 6, and 7 inches three strands of barbed wire are stretched, making the total height of the fence 57 inches. This fence is very durable and inexpensive to build, and it is stated that it will keep out any dog.

Forest Fire Prevention

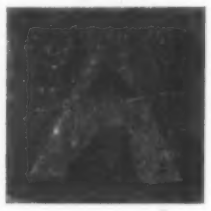
THE prevention of serious damage from forest fires depends largely on prompt information and quick work in checking and preventing the spreading of such fires. In some districts the officials of the Forest Service of the Department of Agriculture have enlisted the interest of residents of their region with excellent results. A recent report of this department states that, in addition to his own fire detection system, the supervisor of the Palisade national forest, Idaho, was notified of each fire by from five to ten different local settlers, who thus showed their co-operation in working for fire suppression.

Recent Advances in Photography*

Some Effects Produced on Sensitive Plates by Material Emanations

By Henry Leffmann, M.D., Ph.D., Member of the Institute

ALTHOUGH for some years past the labors of many expert photographers have been largely directed to the so-called "artistic" side of photography, the purely technical and scientific sides have not been wholly neglected, and an inspection of the current journals and recent books will show many interesting results. The basic procedures for development, fixing, and toning have suffered but little change during many years. The competitive spirit has led to the introduction of many new developers, but most of these are minor modifications of



Negative produced by aluminium.

the original forms, especially as regards the benzine derivatives, and in some cases the change is in name only. A conspicuous instance of the latter is in the use by the Lumière Company of the terms "quinomet" and "metoquinone" in their formula for color-plate work, the information at hand indicating that the names refer to the same substances and that this is merely an intimate mixture of metol and hydroquinone.

Probably the most important, at least the most generally exploited advance in procedure in this field is color-photography. It is curious to note that in the first edition of his work on methods of photography M. Carey Lea stated that, in his opinion, color-photography was impossible, and he gave his reasons for this view. Not long after he modified his view, in consequence of the results of research, so far as to admit the possibility of such photography. He would surely be interested in some of the plates made to-day by the several processes that are extensively employed, and yet he could truly say that the results obtained by the Lumière, Dufay, and Paget plates are not really "color-photography," but colored photographs in which the hand of the artist has been substituted by ingenious automatic processes.

One of the latest applicants for favor in this line is the Paget plate. This utilizes a color screen involving the same principle as the older methods, that is, an even distribution of the three colors; but in the Paget plate this screen is detachable and hence may be used for many plates. Moreover, the reversion of the image as followed by the older methods is substituted by using the plate as a negative and attaching a viewing screen to a positive made in the usual manner. Paget screens are much more transparent than Lumière screens, and hence more satisfactory for lantern demonstration; on the other hand, they are much coarser, and a considerable magnification, as when a slide is projected on large extent, shows the individual colors and destroys the illusion. The total cost of a complete Paget lantern slide is slightly higher than that of a Lumière, but if the operator has a considerable proportion of failures Paget methods are more economical. In sizes of the

Negative produced by Eastman flashsheet.

individual color areas the Paget screens agree closely with those of the Dufay and Thames plates (the latter is now out of the market).

It is well known to working photographers that the Lumière process employs the principle of reversion of image, as the transparency obtained by fixing after first development shows all colors complementary. Working with the color-plates suggested to me to try some of the reversal processes with ordinary plates, that is, to obtain positives by one exposure. In the early days of negative making, the so-called wet plates, the possibility of direct positives was investigated and several methods were devised. These can be applied to the ordinary dry plates with much success, and, although such procedures are of no great practical value or wide application, they afford an interesting field for experiment by amateurs.

My experience has been principally with the following method:

Good exposure and good development are given, the developer is washed off and the plate stood, coated side outward, in a developing dish of black material (or the plate is backed by black paper), and several inches of magnesium ribbon are burned a few inches from it. The silver deposit acts as a negative and the unchanged silver compounds are light-struck. The plate is then immersed in a solution of potassium dichromate and sulphuric acid, such as is used in reversing Lumière color-plates, until the image has practically disappeared. The plate is washed for a minute or so, immersed for a few minutes in a 5 per cent solution of sodium sulphite, and then returned to the developer. The second development gives, of course, a positive, which is then fixed and washed as usual. Theoretically, no fixing is needed, but, in practice, it is best to put the plate through the regular "hypo" solution. The second development will usually be rather slow. It must also be borne in mind that plates well wetted by developer are much less sensitive to light than plates in the dry condition; hence,

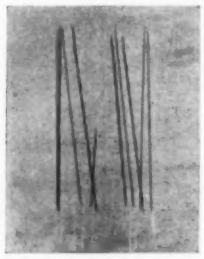
Relative opacity of common writing materials to rays from luminous paint.

if the first development has given a dense picture, a good length of magnesium ribbon should be burned within a few inches of the plate. Care must be taken not to look at the flame and to hold the ribbon with a pair of tongs. All operations are conducted in the dark room. The process will be found useful for making slides in an emergency.

Among the striking discoveries in relation to light, or, it will be more precise to say, radiant energy, is the existence of rays and material emanations which are invisible to the unassisted human eye, but affect ordinary photographic films and many other substances and, in some cases, pass freely through objects opaque to ordinary light.

Upon such phenomena as the X-rays, emanations from radio-active substances, ultra-violet and infra-red light I need not dwell here, as ample discussion of these is to be found in recent literature. I want, however, to call attention to a less exploited field, although, even in this, the basic procedures are not very recent. I refer to what has been called "picture-making in the dark," inasmuch as the etymology of the word "photography" prevents us from applying it to any processes but those in which light takes part. I need not stop to give any elaborate history of the investigations. An early contribution was that by W. J. Russell before the Royal Society (*Science*, 1900, 11, 487), who tried many substances and obtained curious results from some of them. M. I. Wilbert (*Journal Franklin Institute*, 1900, cl, 388) repeated some of Russell's experiments with success. In reviewing these papers, however, it seemed to me that in many cases the impression on the photographic plate might be due to pressure or mere physical contact rather than an emanation tangible or intangible. I wish to say, frankly, that I have failed to obtain many of the results reported by Russell and Wilbert, even though following their methods as closely as the descriptions available enable. In the later series of experiments I adopted the plan of interposing thin paper, usually the ordinary lantern mat, between the object and the sensitive plate, thus preventing actual contact. This method materially retards the action.

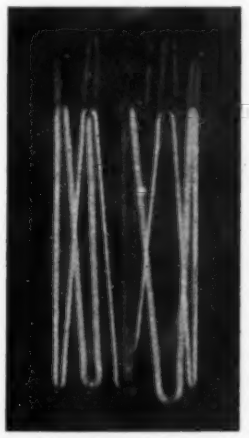
It has seemed to me that the procedures that I intend to summarize briefly may be called skotography (from Greek *skotos*, darkness). One of the most active skotographic surfaces is obtained by scratching or polishing common zinc battery plates. Thin zinc sheets do not seem to be so active, nor does sand-papering the zinc plates yield very good results. If one scratches on



Tungsten filament. Ordinary plate. Positive (reversal).

a tarnished and corroded zinc plate a design, by means of a sharp steel tool, and then lays a dry plate (I used ordinary lantern plates) upon this, with a thin paper between with some perforation or a cut-out design, and allows this combination to remain in the dark for a couple of days, a distinct impression will be made on the sensitive plate that can be brought out by ordinary development. Often the metal is active enough to produce a marked effect in a few hours. Russell expressed the opinion that emanations of minute amounts of hydrogen dioxide produce the effects and that the dioxide is in some way formed by the fresh surface of metal, but I have not yet obtained any information to enable me to form an opinion as to the cause. Magnesium and zinc seem to be specially active; thin sheet iron did not produce any effect. Aluminium has moderate activity. I obtained a strong effect with the so-called "flash-sheets" manufactured by the Eastman Kodak Company. Bibulous paper impregnated with old oil of turpentine gave no appreciable effect, but impregnated with commercial solution of hydrogen dioxide and allowed to get nearly dry before testing gave a feeble picture.

Some experiments have been made on the photographic and other properties of commercial luminous paint. This seems to be composed largely of calcium sulphide in a fixed oil. I have used it on glass plates and have found the most convenient way to get an even coating is to dilute the material with gasoline and pour



Tungsten filament. "Hydra" plate. Positive (no reversal).

the mixture, as was the custom with the collodion used in the old wet-plate process. When dry the paint forms a somewhat granular, slightly cream-colored film, which will emit for some time a soft, bluish light after a few seconds' exposure to any ordinary source of light; but the most satisfactory results are obtained from sunlight and arc light. Incandescent electric lights, even powerful tungsten lamps, are somewhat slow in action.

Many interesting experiments may be performed with

* Abstract of remarks made at the meeting of the Section of Photography and Microscopy of the Franklin Institute and published in the *Journal* of the Society.

the coated plates. If a piece of ordinary blue and ordinary red glass are placed upon a luminous-paint surface and then exposed to the direct light of an arc lamp for about half a minute, it will be found that under the blue glass a strong luminosity is developed, but under the red glass almost no effect is produced. A still more curious effect is that if the entire surface of a coated plate is made luminous and then a portion covered with red glass and again exposed to the light, that part under the red will rapidly lose luminosity. The effect may be also shown by covering a portion of the luminous surface with opaque paper and holding the plate near the red light of the dark room. A rapid falling off in luminosity will occur over the surface illuminated by the red rays. I do not mean that the luminosity merely appears less in the red light, but it is actually diminished.

Luminous paint may find a practical value in some copying work. Ordinary paper is moderately transparent to it, but writing and printing inks are rather opaque. If, therefore, we render a plate luminous, place it beneath a written or printed page and lay a sensitive plate upon the page (these operations being, of course, carried out in the dark), it will be found that an exposure of ten or fifteen minutes will suffice to make a latent image on the sensitive plate which can be developed in the usual way. Experiments with gelatins colored with artificial colors have shown that the rays from luminous paint pass freely through violet and blue films, but scarcely at all through green and red films, and therefore no great difference is noted in the negative obtained when such screens are used with ordinary and with panchromatic plates. The panchromatic plates furnished by the Paget Company, which cannot be developed in red light, and the common lantern slide plates, which may be used freely in red light, gave nearly the same negatives. A Lumière color-plate was also tried; it was found that no appreciable effect was obtained through the green and red films.

So far as I have experimented, the emanation from luminous paints goes equally through glass, quartz, thin films of celluloid, gelatin, barite, calcite, and kyanite, but this phase of the problem is still under investigation. A photographic novelty of practical value is the Paget "Hydra" plate. The emulsion of this contains some hydrazin sulphate, a synthetic product which materially controls over-exposure. I have made tests of the plates by exposing them to the light of a 60-watt tungsten within a few inches of the camera for ten minutes. This is sufficient to produce complete reversal, by over-exposure, in an ordinary plate, but reversal did not occur with "Hydra" plate. Several important uses of such a plate will occur to practical photographers.

Health Requirements for the Aeronaut*

*[The letter here published in the form of a much condensed translation was delivered before the Wissenschaftliche Gesellschaft für Flugtechnik, of Berlin, and is published in the latest yearbook of that society. The same year-book contains a valuable paper on "The Eyes of the Aeronaut," by Dr. Halben, while the preceding volume contains a useful résumé of the physiology and pathology of aeronautics, by Dr. Friedländer. We regret that we have not space to publish complete translations of these three memoirs, as there is comparatively little literature in English on the medical and physiological aspects of aeronautics, important as these subjects are.—*EDITOR.]

In a vehicle of any kind, whether of land, water, or air, the safety of the occupants is more or less dependent upon the physical efficiency of the driver. The degree of such dependence varies, however, with the kind of vehicle. Thus on a locomotive the place of a disabled engineer can generally be taken, for the time being, by the fireman, while in a dirigible airship, the pilot can be replaced by some other member of the crew, though such substitution is an unsatisfactory makeshift, especially because the man who is disabled is himself likely to require the care and attention of at least one other person. On the other hand, in the case of the automobile and of the aeroplane, there is generally no question of substitution in an emergency; a very slight degree of physical incapacity on the part of the driver or pilot is likely to be attended with fatal consequences to himself and his companions. In an ordinary balloon the pilot has generally one or more companions, but these are not often fully competent to take his place.

So far as these considerations apply to aeronautics, the only possible safeguard against accidents due to the disablement of the pilot is a thorough medical examination of the latter before he obtains his license and the imposition of rigid requirements as to health and physique. Until a few years ago German air pilots were not required to pass any medical examination. Even now the examinations prescribed by the different societies are by no means uniform, nor is there any well-defined consensus of poinion on the subject. Hence the following attempt to outline the essentials under this head.

The examination should take account, first, of all conditions that might lead to functional disturbances under ordinary terrestrial conditions, and, second, to those that, while not likely to give rise to serious derangements under such conditions, might nevertheless do so under the more exacting conditions that obtain in aeronautics. Some useful hints as to points to which special attention should be directed may often be furnished by the family history. Thus the following questions are absolutely necessary: Has there been in the applicant's family any case of insanity? Epilepsy, or other nervous disease? Alcoholism, Suicidal mania? Tuberculosis? If the answer is affirmative, an especially rigid examination must be made of the applicant's nervous system or lungs, as the case may be, and of his personal history with respect to these organs.

In making inquiries as to the medical history of the applicant himself, it is advisable to ask leading questions and to aid the applicant's memory as much as possible, as it is easy under such circumstances to forget or pass over, as unimportant, details of great significance. The following questions will be in order:

1. What illnesses have you had?
2. Have you ever had articular rheumatism, hemoptysis (spitting of blood), hematemesis (vomiting of blood), angina pectoris?
3. Have you ever had any mental or nervous disease, especially one involving lapse of consciousness or attention, etc.
4. Have you ever been injured, especially about the head; broken a bone; suffered a sprain or dislocation? Have you ever undergone an operation?
5. Have you any disease of the eye or the ear?

The answers to these questions should be signed by the applicant.

The portion of the examination blank reserved for the personal observations of the physician usually contains the questions, "Is the applicant known to the physician? Has he ever been treated by the physician, and for what?" To these I would add, "Has the physician ever made an air-voyage with him?" Other questions to be answered by the examiner are:

Does the applicant give the impression of a strong and healthy man?

Physical strength is essential, as the task of the aeronaut, whether in an aeroplane, dirigible, or ordinary balloon, is laborious under normal circumstances, and may become extremely so, as, for example, when the occasion arises to handle sacks of ballast rapidly.

Are his limbs in any way weak or defective? Are there any malformations of the bones, and if so do they interfere with the use of the limbs?

Evidently a pilot needs sound limbs, as he may have to climb a rope-ladder, ascend the suspension gear of a balloon, climb down from a tree in which his craft has landed, etc. In case a limb has been injured, the cure should have so far progressed that no special precautions or "nursing" of the limb are necessary; otherwise attention devoted to this object may cause inattention to aeronautical duties.

Is the voice strong? Does the applicant stutter to any marked extent?

In any form of aircraft except the aeroplane the pilot will often have occasion to give rapid orders, which must be distinctly heard and understood.

What is the conformation of the thorax? Are the lungs sound?

The form of the chest often gives a hint as to the state of the lungs. Tuberculosis should debar the applicant, as even in its early stages there is danger of hemorrhage, and also emphysema, which may become dangerous at high altitudes on account of the difficulty of perfect exhalation.

Is the heart sound? Is there any lesion of the blood-vessels?

Among cardiac troubles we must especially look out for weakening of the heart muscles and valvular defects. This follows, first of all, from the mere physical labor that may at times be imposed upon the aeronaut. What is true of the heart is also true of the blood-vessels. Even a moderate degree of arteriosclerosis unfits the applicant for such exertions, moreover, aside from the danger of rupturing the vessels, this disease, by diminishing the caliber of the arteries, causes an insufficient supply of blood to the brain; the reduction in the oxygen content of the air at high levels promptly aggravates this condition and disturbs the mental functions, causing dizziness, uncertainty of movement, forgetfulness, etc.

As to the effects of altitude on the circulation and breathing, it is not necessary to insist upon the attainment of excessive heights, at which the pilot would be expected to resort to the inhalation of oxygen. Provided the apparatus works properly, this process is not attended by danger and high flights under such conditions make no special demand upon the physique. On the other hand, it is necessary to insist upon the ability to ascend to heights of from 10,000 to 13,000 feet without the use of oxygen, and without any diminution of efficiency. The balloonist must expect at times to be carried up to such heights; either on a sunny morning after a nocturnal ascent, or in ascending air-currents. I myself recall having been carried up almost instantaneously, during a thunderstorm, from a height of 3,300 feet to over 15,000 feet, while my companion sat in the violently swinging car, helpless with seasickness and altitude sickness. I have heard of cases of altitude sickness at 6,500 feet, with nausea, shortness of breath, and livid color, while in one case breathlessness and *malaise* began at half this altitude. Evidently a pilot suffering with altitude sickness cannot be depended upon to perform his duties efficiently. In order to test the applicant in this respect, a trial ascent to at least 12,000 feet should be required.

We next come to the abdominal organs. Under this head I think it may be stated that any condition that causes pain or discomfort should be regarded as prohibitive, on account of its effect in distracting the aeronaut's attention from his duties. Hernia is prohibitive only in case it interferes with freedom of movement.

Chronic disease of the middle and inner ear must be considered prohibitive, at least in the case of aviators, especially on account of its impairing the equilibrating function of the ear (i. e., of the statolith). Stubborn catarrh of the Eustachian tube may give rise to much discomfort on account of rapid changes in atmospheric pressure, but I should hardly go so far as to reject an applicant on this ground. Marked deafness I consider prohibitive.

We now come to the most important class of defects, viz, those of the nervous system. Under this head we may consider separately those derangements that merely affect the motor and sensory apparatus, and those that involve mental disorder. Derangements of the first class are easy to detect according to well-known methods. We can test for paralysis or muscular atrophy, examine the reflexes, look out for muscular twitching, etc. All nervous affections that impair the strength or accuracy or bodily movements should be considered prohibitive. We can never be sure how rapidly such conditions may become intensified. Thus, for example, a very slight degree of tabes (incipient locomotor ataxia) is a sufficient ground for rejection.

Among mental affections, those characterized by melancholia need hardly be considered, as the patients in such cases are not often desirous of becoming aeronauts. Far more dangerous is the opposite condition of exaltation, or hypomania, which is often difficult to recognize from external indications and which may lead to impulsive and reckless acts.

Incipient paralysis is especially dangerous. Every possible precaution must be taken to exclude such cases; whenever suspicion exists, not only should the medical history of the applicant be thoroughly investigated, but decision should be reserved and a reexamination made after a lapse of time. As paralysis is so commonly a result of syphilis, and as it may be extremely dangerous even in its early stages, when diagnosis is difficult, I am inclined to apply the same rule to the aeronaut that Dr. Placzek has laid down for railway employees, and exclude all syphilitic applicants, though I admit that this is rather sweeping.

Mild cases of epilepsy are not susceptible of diagnosis in the intervals between seizures; they can therefore only be detected by a careful investigation of the medical history.

Alcoholism, the more severe forms of which can be readily diagnosed, is absolutely prohibitive.

We have lastly to consider a variety of nervous disorders which are, as a rule, easily detected and recognized in ordinary medical practice, when the patient voluntarily confides his symptoms to the practitioner, but which are by no means so easy of diagnosis when the patient is undergoing examination for a pilot's license and is anxious to avoid rejection. These include neurasthenic and hysteric conditions, as well as ordinary "nervousness." They can be detected only by means of actual ascents under an experienced pilot, who should report all abnormal actions on the part of his pupils to the medical examiner; the latter will decide what importance should be attached to them. Most aeronauts are more or less nervous and upset before an ascent. Seasickness, when due to the imagination rather than to the actual movements of the balloon-basket, excessive timidity, strong aversion to looking downward, the desire to jump overboard, and hallucinations of various sorts, are all to be considered in deciding upon the fitness of the applicant. Whether any of these common manifestations should be considered prohibitive in certain cases must be left to the judgment of the physician, who should preferably be himself an aeronaut.

* Abstract of a letter by Dr. E. Koschel.

"Suction" Between Passing Ships—II*

Important But Little Understood Forces Affecting the Motion of Vessels

By Sidney A. Reeve, M.E.

Continued from SCIENTIFIC AMERICAN SUPPLEMENT No. 2036, Page 32, January 9, 1915

THE form in which the stream-line ship, derived mathematically from sources and sinks, was left by Mr. Taylor's last addition to its theory was still unsatisfactory for the analysis of suction between actual ships. Its outline was everywhere convex. Stem and stern were both rounded, there was no hollow entrance or run, and the middle body was not parallel. While such a ship form might do for the study of the general principles (which, indeed, could be well understood without any stream-line analysis), yet for any estimate of *quantitative* aspects of the problem a better ship-form must be had. The magnitude and exact form of the constrained wave accompanying a typical steamship could be had only by mathematical deductions which were based upon a typical ship model, having a long parallel middle body, hollow entrance and run, and sharp prow and stern.

To the writer it appeared likely that Taylor's method might be made to yield these results, provided a suitable function of sources and sinks could be found to form the premises. This could be determined only by the cut-and-try method, which, as each try involves tedious repetition of simple computations, proved to be laborious. However, the nineteenth function tried gave a fairly satisfactory model, as may be seen from Fig. 7, the ship-form in which is a mathematical stream-line and not an assumed form, and upon that model all the curves and contours presented here are based.[2]

Taylor's method was modified slightly in order to facilitate the work of investigating many different premises, by taking a large number of points and relying upon averages, rather than integrating a separate curve for each one of the thousands of points investigated; yet even with this the method is laborious. The ship's length was divided into twenty parts, and in terms of these longitudinal units, and for each one-fifth the beam extending laterally, the stream-lines were computed over a sea-surface extending nearly a ship's length forward of the stem, and nearly two ships' lengths abeam. In this work only one quadrant, from the course ahead to either beam, need be computed, because the mathematical treatment makes the four quadrants symmetrical. But in actuality the viscosity of the water and the energy lost in wave-making make the constrained wave astern smaller than that ahead.

This plan would apparently give over two thousand observations in each quadrant, but in the regions farthest from the ship many points were omitted, the contours there being of less curvature. Taylor's intermediate curve was plotted on a sheet 4 feet square, the largest practicable in this case, and final values of stream-line widths were computed to four decimal places (preceded by a characteristic), with intermediate computations carried to five or six places. Students repeating this work will do well to plan for greater accuracy than this, for whatever scale is adopted at the start must be adhered to throughout the work. The stream-lines were all drawn as accurately as possible, but the computed values were charted thereon and formed the sole basis for deduction, no values being taken off graphically, except in the use of the "intermediate" curve, where precautions were taken to have the graphical work sufficiently accurate.

Fig. 7 gives the results for a single ship. The model has a beam one-eighth its length, with a parallel middle-body of half its length, sharp stem and stern, and slightly hollowed entrance and run. The draught was taken as four-tenths the beam. The curves are true for a relation of speed to length indicated by any of the following proportions, from which other lengths or speeds can be interpolated; but the numerical scale of elevations will vary with the square of the speed, that noted in Fig. 7 being true only for an 800-foot ship going at 20 knots.

Length, ft...	200	300	400	500	650	800	1000
Speed, knots..	10	12.35	14.14	15.82	18.03	20	22.37

The curves of Fig. 7 are level contour lines. For readers not familiar with the preceding article, it may be repeated that these contours show not actual elevations or depressions of the sea, but surpluses and deficits of sea-pressure in an imaginary sea having just the depth of the ship, and confined by a thin sheet of rigid ice on its surface. These are assumptions necessary for practicable mathematical treatment, but it is obvious that, while the vertical release of the water under actual conditions would vary these curves somewhat, yet their relative disposition cannot be far from those shown.

Through the center of the ship's displacement are drawn the 45 degree lines which would be its mean sea-level contour if its displacement were reduced to a single point. Asymptotic to these lines are the approximately hyperbolic mean sea-level contours due to the elongated form of the ship. These hyperbolas extend indefinitely away from the ship, but all the other contour-lines are closed curves, or would be if the diagram were sufficiently extended.

Between the limbs of each hyperbola, ahead and astern of the ship, rises an oblique cone of water at stem and stern, reaching an altitude of 8 feet above mean sea-level. This altitude, it is to be remembered, is *exclusive* of the bow waves or stern waves. It is relatively to this conical surface that these waves raise their crests and depress their troughs. While the peak of this constrained wave is sharp, its base is very broad. At a distance of half a length ahead of the stem the sea is elevated 3 inches over an area extending more than a length athwart the course. But on either beam the depression of the sea is still more marked in extent. For five-eighths of a length out away from either rail the sea is a foot or more below normal level, while the 3-inch depression extends out to a distance probably of several lengths, not measurable on the diagram. It is this wider area of depression than of elevation which probably has given the name "suction," rather than "repulsion," to the phenomenon. It is noticeable that the deepest depression does not occur directly abeam, but on either bow or quarter, where occurs a pocket reaching a depth of 9 feet. From this point the water-line rises to less than 5 feet below normal, sinks again to more than 5 feet, where the curvature of the bow alters to the straight lines of the middle body, finally rising to less than 4 feet at mid-length. The peculiar form of the 4-foot curve led to its being checked by a fresh computation for that district upon a larger scale; but the result only confirmed the earlier curve, strengthening the writer's confidence in the virtual accuracy of the entire diagram.

The symmetry of these constrained waves as between bow and stern depends not merely upon the assumption of perfect fluidity in the sea-water, but upon levelness of sea-bottom. Should the ground beneath the ship be rising, this constrained wave will be in process of formation, to the magnitude shown, from the smaller constrained wave of deep water; and this formation must take place first at the bow. This explains how it is that a high-speed boat, such as a destroyer, will feel a bank many feet beneath its bottom, so forcibly as to fetch the crew up against rails or bulkheads. As the bow passes over the bank this cone of water must form ahead, while the stern has yet no corresponding cone pushing it ahead.

These contours also explain how it is that a ship's resistance is so markedly increased in shoal water. It also explains how a high-speed boat seems to settle in the water. One case is reported where a paddle-steamer failed to come to speed on her trial trip, it being found finally that her paddles had not been located with a view to this depression of the water on either beam, so that before she attained full speed the floats lost their full contact with the water. In all ships of appreciable speed having a painted water-line the uncovering of the underbody throughout the greater portion of her length by the trough of the constrained wave is plainly visible. The heaping of the water about her stem is more or less obscured by the bow-waves; but at the stern, although the stern-crest is smaller than the bow-crest, the re-gathering of the water under the counter is usually plainly to be seen. The importance of a fine run for taking advantage of this phenomenon is apparent when it is noted that in the model of Fig. 7 the amount of energy constantly being stored in the constrained wave is some 40,000 horse-power. The bulk of this power is being as constantly regained at the stern, but because of the viscosity of the water, not all of this enormous supply comes back. It is the office of the ship's run to regain as much of it as possible.

III. INTERACTION BETWEEN TWO OR MORE SHIPS.

The problem of determining the distortion of the sea-surface in the neighborhood of two ships moving on parallel courses, and overlapping, which is the standard position for the development of suction forces, is impossible of exact mathematical solution, so far as the writer is aware, even with the aid of the imaginary sheet of ice and the restriction of the water to motion in horizontal directions. But the stream-line method offers at least two ways of securing approximations to the truth which are near enough to shed very much more light upon the situation than is available in our present state of knowledge.

The first method consists in taking two sets of stream-lines identical with Fig. 7, overlapping them in the relationship between the two vessels which is to be investigated, and then adding them.

The result of such a procedure is shown in Fig. 8, which is derived from two ships like Fig. 7, placed on parallel courses a half-length apart, with one ship lapping a quarter-length over the other. The resultant stream-lines and sea-contours are quite accurate, but they have now lost their practical value as being founded upon ship-models fairly typical of actual ships. For the models secured by this mathematical method are themselves fluid (until we imagine them momentarily solidified), and therefore each ship-model has now become warped, narrowed, and distorted by the influence of the sources and sinks forming its neighbor. The beam has been reduced about one-half, the axis of the model is no longer straight nor co-incident with the intended course, while the general heading of each model has been sheered away from its neighbor.

All of these features tend to minimize the mutual hydraulic reaction due to the solidity, in actual practice, of the neighboring hulls which is called "suction." Nevertheless, Fig. 8 shows plainly, in comparison with Fig. 7, the general directions in which the sea-contours about each ship are modified by the presence of the other. First, following out the 45-degree lines from the center of displacement *C* common to the two ships, it is evident that the area of superpressure included in the "hyperbola" ahead of the leading ship must be unsymmetrically placed. All the curves depicting the cone of superpressure ahead of the leading ship corroborate this view. Her head is being steadily pushed to port more than is visible in the small scale reproduction of the curves.

But it is also true, as could not be predicted from the general disposition of the 45-degree lines, that the head of the following ship is also being pushed to port by a cone of superpressure ahead, which bears much more obliquely than in the case of the leading ship. It is the pressure of this cone of water, into one side of which the following ship is always heading, which crowds her over towards the other's quarter. It is the rapid exaggeration of this cone, both in altitude and obliquity, by the piling up of the water displaced by the leading ship, as soon as the following ship yields at all to its insistent pressure, which explains the suddenness with which suction collisions occur, and the complete unmanageability of the following vessel as soon as she is permitted to head in the slightest degree towards the other.

It is also to be noted that in Fig. 8 the altitude of all four cones of water has been exaggerated, both in comparison with Fig. 7 and with each other; for, since the beam of the vessels has been halved, the cones of Fig. 8 should naturally be only 4 feet high, and the same at each end of the ship. But in Fig. 8 the cones at the overlapping ends are nearly 5 feet, and at the distant ends well over 5 feet, in height, or nearly a quarter larger than proportionate to Fig. 7. At the same time the larger cone constantly retards the progress of the leading ship, while accelerating that of the following vessel.

One of the earliest instances of hydraulic interaction between following ships ("Narragansett"-"Providence," in 1869) witnessed how this last-mentioned feature may lead to the actual towing of a slower consort for considerable distances by the leading and slightly faster of two vessels upon parallel courses in restricted waters, purely by "suction." This is what is called in America a "wireless tow." The phenomenon may not result in collision. In the case cited the "towing" continued for several miles. The final collision, occurring in Hell Gate, may or may not have been materially aided by suction forces. The action is common in suction collisions, and one of the best, although late, warnings which the pilot of a following vessel may have is the sense that

* Reproduced from *Engineering*.

[2] Students should be cautioned that all writers on this stream-line topic complain of the labor and tedium of computation. The writer appreciated this only after he had become involved. The processes are simple, and anyone in command of a corps of clerks could easily carry the Taylor method to further and better results than are here presented. But anyone attempting to investigate alone, as a spare-time occupation, must be prepared for very slow and tedious progress.

his ship is being "picked up" or accelerated by the faster ship which has almost passed. When this occurs, although collision may not be inevitable, it is a sign that his ship is in unstable steerage, and that the first irregularity of sea-bottom, or projecting pier, or even the approach of some otherwise harmless third vessel, may precipitate a disaster against which all his skill and engine power will be unavailing. But even a slight sheer away from the other ship, taken in time, will suffice to avert it.

One method by which Fig. 8 might be developed into a set of contours or typical ship-models would consist in starting all over again, from initial functions of sources and sinks which would be arbitrarily selected upon curvilinear axes (convex towards each other), and of double strength, so judged that when the two sets of stream-lines were added the effect would be to straighten out the hulls into normal shape, on straight parallel axes. But this method promises an exaggeration of even the laboriousness inherent in the work reviewed thus far. While its fruits would be mathematically exact, and therefore worthy of full confidence, its penalty in the form of labor was too forbidding for the writer's command of leisure, and an easier "side step" was taken around the difficulty.

This consisted in going back to the original set of stream-lines from which Fig. 7 was developed, and superposing them as for the preparation of Fig. 8, but then regarding the two hulls as remaining solid. This method gives very closely the volume of water entrapped by each stem as it cleaves its course, which volume must find its way aft as best it may, constrained by solid hull and sea-bottom and superficial ice. The question then remaining is: Along what stream-lines does this water distribute itself?

This question cannot be answered mathematically, but the guidance provided by the composition of Fig. 8 permits a fair estimate to be made as to how the exact stream-lines would be influenced by the additional water entrapped on each side of each hull by the solidity thereof. The estimate thus applies merely to a minor correction of values already obtained with mathematical accuracy, and, although the form of the contours is influenced considerably by the method of correction adopted, it is thought that the result is fairly accurate. It at least shows accurately the direction in which Fig. 8 would be modified if the hulls were solid; and the amount of correction in this direction might easily be more than that shown in Fig. 9. This diagram I would therefore style a "mathematical conjecture."

The mathematical form of correction must be one easily applied to thousands of points in repetition. The one most readily suggesting itself, in view of this requisite and of the facts become familiar during the preceding work, was to assume that the surplus or deficit of water entrapped by the stem is distributed over the sea-surface away from the ship's side in what amounts to a logarithmic curve, approaching zero as the distance increases indefinitely. That is to say, it was assumed that $1/n$th of the surplus or deficit was retained between the first two stream-lines, $1/n$th of the remainder between the next two, and so on. The factor n was given a different value arbitrarily, in the light of the preceding work, for each different point of the ship's length.

Whatever criticism may be directed at the choice of this particular method of distribution, it is beyond question that the true stream-lines for two solid hulls like Fig. 7 must consist of a distortion or correction of the stream-lines on which Fig. 7 was built, by some factor which has its greatest effect at the ship's side, with a gradual dissipation of that effect with lateral distance which can never become complete. Considering that the uncertainty applies to a minor fraction of a minor correction—the form of the resultant contours depending chiefly upon the particular localities in which a positive or negative correction is appended to a previous positive or negative departure of stream-line from parallelism, rather than upon the magnitude of the correction itself—it is believed that Fig. 9 is worthy of credence, as offering the only approximation to the truth regarding following ships which has yet been offered from even a semblance of a mathematical basis.

The contrasts between Fig. 9 and Figs. 7 and 8 are numerous and interesting. In the first place, whereas Fig. 8 is inevitably symmetrical, from its mathematical origin, in the sense that it makes no difference in which direction the two ships are moving, in Fig. 9 this is no longer true. Owing to the different surplusage of water entrapped by the two stems, which surplus must then travel throughout the length of the solid hull before again exerting its influence upon the water on the other side of that hull, the contours about one hull are appreciably different from those about the other. The oblique cone of water ahead of the following ship is quite different, both in magnitude and angle, from that following the leading ship. The excess of pressure from one quarter over the other, which is the principal force acting to sheer the ship off her course, is much greater in the following than in the leading ship—which explains again how it is that it is usually the following ship which is deflected.

In general, however, Fig. 9, which is really derived by a quite different and independent line of reasoning from Fig. 8, corroborates that diagram. The leading ship is seen to be sailing continuously up-hill, against a cone of water at its stem which is 3 feet higher than that at its stern, while the following ship is likewise sailing down-hill all the time into a relative hollow of equal depth. This is caused by the leading ship's shouldering the burden of displacing the water for both of them, the reaction from which displacement rises under the stern of the following ship, and aids her progress.

In Fig. 9 the pairs of cones of water ahead and astern of the ships respectively are each merged into a single "hyperbolic" cone asymptotic to the 45-degree lines, whereas in Fig. 8 they were separate; but this is because in Fig. 9 the ships lap by four-tenths of a length, whereas in Fig. 8 they lapped by only a quarter length. It is probable that a slightly greater lap than four-tenths would exaggerate the mutual distortion of contours and the development of suction forces, but the labor of computing even a single relative position was so great as to be prohibitive of any comparative investigation of different ones. In Fig. 9, as in Fig. 8, the lateral distance between the two courses is a half-length, but in Fig. 9 the beam of the ships reduces the minimum distance between hulls to about 0.385 of a length. In both diagrams the full-line contours are those drawn through computed points, while the broken lines indicate where the contour was interpolated by estimate.

While Fig. 9 gives the best foundation from which to estimate the effect of variations from its particular arrangement of dimensions of hulls, the results of this particular arrangement can best be seen in the form of profiles of the water-lines of the two ships—assuming again that the computed pressures of Fig. 9 might be translated into actual elevations of water by the removal of the imaginary superficial sheet of ice, without altering the values. Such profiles are given in Fig. 10, in which the vertical scale is exaggerated five-fold for better visibility. They are plotted directly from Fig. 9.

On each profile is shown a conventional center of lateral pressure C, taken at one-fifth the ship's length from the stem. Comparing the two profiles, and remembering that the steering of any ship depends upon the horizontal moment developed between the rudder forces at the stern and this center of lateral resistance C (which is always well forward in the direction of the ship's motion), it again becomes obvious why it is that it is usually the following vessel which is diverted from its course. Until Fig. 9 was developed, this explanation had to depend upon the fact that the following vessel was usually the smaller and weaker. Now, however, it appears that the forces at work upon the after vessel are so much larger than upon the leading ship, even when the ships are of equal size and speed, that the following vessel must still be the one more liable to diversion from her course, even when somewhat larger.

As to speed, it is not evident just what difference it would make as to relative forces. The suction forces are the mutual product of the two ships, acting cooperatively. They are the result of the aggregate rate of displacement by the two vessels, so that any increase on the part of either ship must result in the exertion of increased divertive forces upon both.

Fig. 10 should make it plain why it is that a vessel in the grip of suction forces pays no attention to a hard-over helm. Making every allowance for the exaggerated vertical scale, the length of after-body, far from the center of lateral resistance, which is exposed to a head of several feet of water-pressure acting laterally, is overwhelmingly larger than any lateral force imaginable to be developed by or on a rudder. It is utterly impracticable to build or navigate ships so as to defy suction forces. These forces can only be avoided, and that only by foresight.

But the most important question of all: Why do not suction collisions happen frequently, and why do they happen unexpectedly in situations apparently encountered frequently before in safety? remains to be answered. The answer involves the third wall of the "Venturi" passage-way between the two ships—namely, the sea-bottom, or else some equivalent outlying factor not yet brought into the discussion.

Figs. 9 and 10 are based upon a degree of propinquity which is seldom reproduced in actual navigation, even disregarding our mathematical assumptions of imaginary ice, etc. That is to say, ships seldom move at full speed on parallel courses within half or third a length of each other, in water barely sufficient to float them. The explanation of why suction collisions do not occur is that even a moderate deepening of the water below their draught rapidly moderates the forces at work, so that by the time the water is deep enough to permit full speed, it is deep enough to make fairly close overlapping courses safe. The explanation why suction collisions do occur when they do is usually the fact that a moderate contribution of solidity to the ship's environment, by sea-bottom or by a third vessel, or an accentuation by even a slight convergence of courses, is competent to exaggerate back into control these forces which the previous depth of water had rendered innocuous.

For deep water, while reducing the intensity of the constrained wave, correspondingly increases its volume or extent: and it also permits increased speed on the part of the ship. It is when the ship passes suddenly from deep to shallow (or otherwise limited) water that trouble arises. This great volume of constrained wave, moving at a speed above that attainable by the ship in uniformly shallow water, piles up into a degree of intensity which it could never assume in uniformly shallow water. If it begets disaster, it does so by suddenly releasing an enormous accumulation of energy which the ship has been carrying with her, in addition to that currently at work.

A glance at Fig. 9 will show how rapidly all the con-

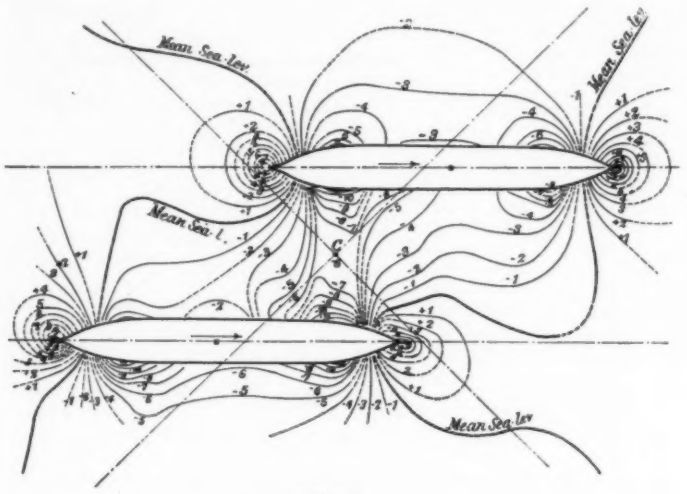
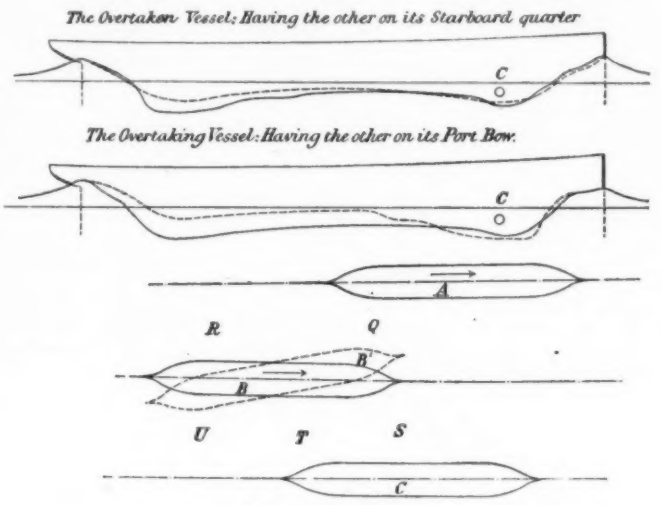


Fig. 9.



Figs. 10 and 11.

ditions creating suction will be exaggerated by any angular swing of the following ship toward the leading one. The two pockets, 11 feet deep, on her port bow and starboard quarter are there because a surplus of water is trying to get to its ultimate destination, which is off her port quarter, as rapidly as possible. To give it the necessary horizontal acceleration a drop in head must occur. But how much more will this region off her port quarter lack its supply of water if the bow swings toward the leading ship, cutting off the channel between them, while the stern swings out, lengthening that route for the water?

But what most commonly happens is that while the courses remain parallel the ships run over a hump in the sea bottom, or they pass a pier-head or bank, which, when opposite the trailing ship's starboard quarter, reduces the pathway for water, and therefore deepens the pocket on that portion of the hull—the former being the more common cause of disaster. Such a combination of circumstances will instantly exaggerate into uncontrollability suction forces which previously—perhaps during a considerable period of overlapping—had proven harmless. It may be that the vague fear of suction, so often mentioned by pilots as felt when steering such following courses, had been stilled by such preceding period of security, only for the relaxation of alertness just at the time when the alteration in the depth of water created the real danger. Too many suction collisions occur just where the water shoals slightly to permit disregard of this factor, although it is impossible to bring it into any mathematical discussion. Nor are the data of actual disasters sufficiently accurate so that the shoal can often be located exactly, in relation to the ships, when action occurred. But it is seldom that unquestionable suction occurs when only the two ships are present, surrounded by open water.

A similar contributing factor in many of the minor cases occurring in American inland waters is a third vessel. Fig. 11 shows how such a combination may produce disaster. *A* and *B* are ships moving in the same direction on parallel courses and overlapping. *C* is a third ship upon another parallel course. If *C* be moving in the same direction as *A* and *B*, *B* is plainly (from Fig. 9) steering in very unstable equilibrium, sailing steadily down-hill into the pocket created by the displacement of *A* and *C*. The slightest swerve of *B*'s head towards *A*, as at B^1, will not only starve the *Q* region of water needed at *R*, as would be true if *C* were not present, but will trap an over-supply of water between B^1 and *C*, exaggerating the pocket at *U* and creating an additional one at *T*. *B*'s head will be crowded towards *A* and her stern and middle body drawn towards *C* with irresistible forces, which grow stronger with every additional degree of swing.

Not only are there several instances where this situation has led to disaster after two of the three ships (say *A* and *B*) had sailed closely but safely for some time, but there are a few cases where the only plausible explanation of an otherwise inexplicable sheer of *B* into *A* was that *C* had exerted this same effect, even although passing in the opposite direction. These are all in the restricted channels connecting the Great Lakes, used by boats of heavy tonnage; but the speed was correspondingly low, a circumstance minimizing the danger of suction.

Only two attempts have been made, to the writer's knowledge, at the experimental determination of suction forces. One was the Taylor experiments with models in the Washington towing-tank already mentioned. These observations confirmed in every way the hydraulic theory already presented. They were limited in their educational value, however, by the fact that the models were fixed relatively to each other, thus preventing any knowledge as to how rapidly these forces are enhanced by the natural swing of the vessels towards each other. Any determination of this acceleration will, of course, be very difficult; but since it is quite impossible by mathematical analysis, and is virtually impracticable even with models (because the prime question is whether such action occurs more or less rapidly, in full-size ships, than the human organism can act), it is unfortunate that the ideal ships of the British or United States navies cannot be utilized for this purpose. When suction was the thing anticipated and under measurement, it would be easy to make the experiment safe.

The other known set of experiments was made with actual vessels, if small ones, by Prof. A. H. Gibson and Mr. J. H. Thompson (reported in *Engineering*, September 27, 1912). These experiments also corroborated hydraulic theory in every respect. Their chief drawback was the inequality in length between the two boats, as well as their small size. One, a steam yacht, used as the leading vessel, was virtually three times the length of the other, a motor boat. From Fig. 9 it will be obvious that it is important that the two ships be at least similar in length. In one prominent suction collision, however—the "Brockton"-"Hartt"—the leading boat was much larger than the attracted boat; and in this instance, too, arose a beautiful example of interaction between the contrained wave of the smaller boat and the echelon wave of the larger (the two waves being of similar length) as the contributing cause which converted an otherwise safe situation into a disastrous one. This collision is further the more remarkable in that it occurred in comparatively deep water, outside Sandy Hook, between vessels of comparatively light draught.

There will be given a brief description of a few of the more prominent or illustrative suction collisions. Anyone who has studied the entire list of such recorded disasters will be impressed with the clear way in which all but a very few are explicable as natural, if not inevitable, phenomena, so soon as the foregoing hydraulic theory is applied. But he will also be impressed with the variety of combinations of circumstances which may arise to create suction forces powerful enough to produce disaster, and thus to record themselves in civil suits at law. These combinations are too many to be always remembered, nor can the list of possible ones be already exhausted. We have not yet finished with suction disasters. While channels are continually being deepened, longer, deeper, and more powerful hulls are appearing at an even higher rate of progress.

For these reasons the subject cannot be dismissed without at least a word as to the possibility of preventing future suction collisions. The general education of pilots is one obvious measure of relief, and it is to that end that the present article is primarily aimed. But another more formal and reliable preventive is feasible, in the writer's belief, and this is an amendment to our international navigation laws covering the case of overtaking vessels. The suggested amendment (which is offered tentatively, by one who is not a certificated navigator) should read to this effect:

That when an overtaking vessel asks for and receives from the leading vessel permission to pass, by the exchange of the usual signals, the acquiescence of the leading vessel shall enforce upon both vessels the slowing of their engines, those of the following vessel being reduced to "half speed," and those of the leading vessel to "slow," which condition is to be maintained until the overtaking vessel's stern has passed clear of the overtaken vessel's bow. In restricted waters such a procedure is the only safe alternative to the overtaking vessel remaining astern until wider sea-room has been reached.

(To be concluded.)

Correspondence

[The editors are not responsible for statements made in the correspondence column. Anonymous communications cannot be considered, but the names of correspondents will be withheld when so desired.]

The Inductance of a Coil

To the Editor of the SCIENTIFIC AMERICAN SUPPLEMENT:

IN the SCIENTIFIC AMERICAN SUPPLEMENT of October 10, 1914, appeared an article by Kenneth Richardson, in which was given a formula for the calculation of the self-inductance of a coil.

I wish to call attention to the fact that the formula as given is, in general, inapplicable, and of no practical use for the purpose stated in the article. It is "exact" only in case $\frac{D}{l}$ = a certain value, where "*D*" is the diameter, and "*l*" is the length of the coil. The quantity "101.5" has a different value for every change of $\frac{D}{l}$. Using *K* for "101.5" the formula may be written:

$$L = \frac{N^2T^2}{l} K$$ where:

L = Inductance in centimeters.
N = Total turns.
T = Radius, centimeters.
l = Length.
K = A constant, depending in value upon $\frac{D}{l}$

The calculation of the constant *K* is a long and difficult matter and beyond the ability of the average amateur radio experimenter. Likewise, the calculation of the self-inductance of short solenoids is by no means simple when accurate results are required.

To illustrate by an example the error if one uses the formula given by Mr. Richardson: I shall take a coil which would be suitable for use as the primary of an oscillation transformer for amateurs. Dimensions of coil:

D= 20 centimeters, or 8 inches (approximately)
T= 10 " " 4 " "
l = 10 " " 4 " "
N= 8

where:

D= Diameter
T= Radius
N= Total turns
l = Length

Calculating the inductance of the above coil by Nagaska's formula we get: 13.277.6954+ centimeters, or 13.2776954 micro-henries. This value is extremely accurate, as checking with the absolute formula of Lorenz, the difference is only 0.057+ of one centimeter.

Calculating the self-inductance of the same coil by the formula: $L = \frac{101.5\ N^2T^2}{l \times 1000}$ we get 25.984 micro-henries, as the approximate self-inductance.

From the foregoing it is evident that the error for a coil in which $\frac{D}{l} = 2$ is approximately 95 per cent.

In order to have gotten highly accurate results we should have had to use 51.865+ instead of "101.5," in the case of the coil which we considered where $\frac{D}{l} = 2$.

In the above case the self-inductance is the current sheet value and requires a correction, depending upon the size of wire used, for the true low frequency inductance. If we use No. 4 B. and S. wire, the self-inductance will be 1.0546662 micro-henries greater, approximately. The low frequency inductance then is: 14.342+ micro-henries. Knowing the self-inductance exactly, the method of obtaining the dielectric constant as described by Mr. Richardson is simple and should be suitable for amateurs. But the self-inductance cannot be calculated with the required accuracy by the formula given, excepting, as before stated, in the case of a coil of certain dimensions.

J. E. FREDERICKS,
El. I. C. Radio, U. S. N.

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